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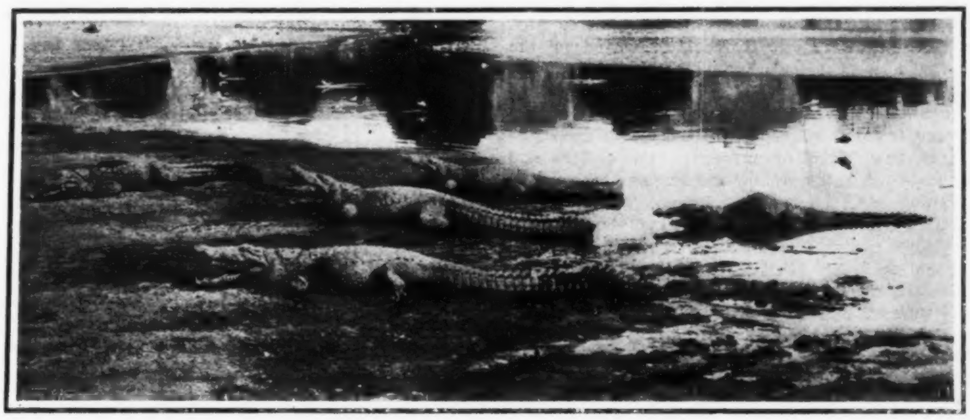
PHOTOGRAPHING ALLIGATORS.*

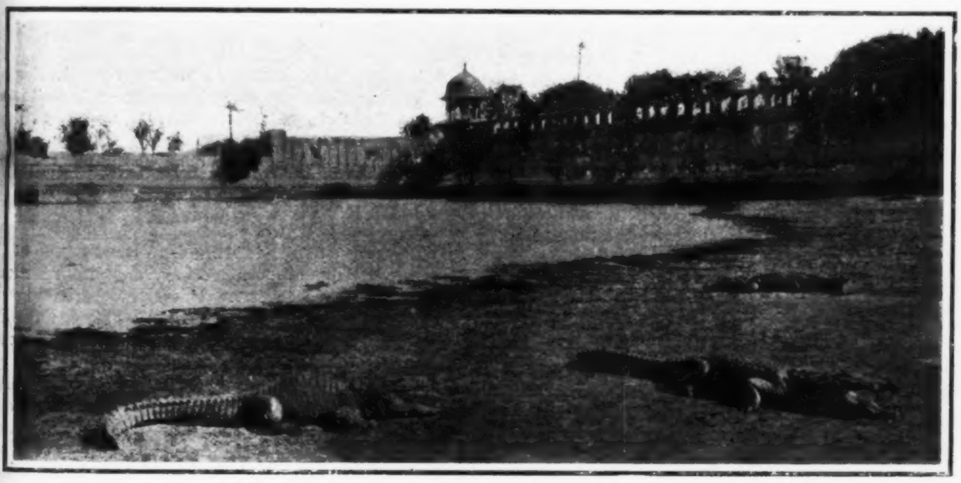
By Herbert G. Ponting.

It was my cherished ambition to secure a decent photograph of an alligator when I went to India. Not the confined article; I did not want that. I wanted him in his native element; and when I heard there were plenty of them in the lakes at Anaradjhpura in Ceylon I anticipated that there I should secure the picture I so coveted, as well as views of the ancient dagobas and buried palaces of this city, whose glories now lie masked under a thousand years or so of tropical growth. Dagobas and ruined palaces I photographed to my heart's content, but I found that the alligators had very pronounced ideas of their own on the subject of photography. Alligators there were in plenty. The lakes literally swarmed with them, but when I finally left Anaradjhpura it was as a wiser and a disappointed man. I had not been able to get near one. Even though I had long-focus lenses and telephoto lenses and everything I needed for the work, I did not get a picture. Time after time I went and waited and waited and crept and crawled and stalked my prey; but the moment I got anywhere near him and showed myself he was off like lightning into the water, and only an eddy moving along the surface with the speed of a motor-boat traced his whereabouts below. I finally gave it up. I left the camera behind and took a rifle instead, and even then it was only after long and careful stalking that I killed my first at a range of about 250 yards; it was impossible to get any nearer. These lake alligators are not like the lazy brutes which lie on the banks of certain rivers ignoring passing steamboats. They scent danger from afar, and on the slightest sound or movement detected the apparently lifeless bit of waterlogged stranded wood is off like a flash of light. People have said to me, "Why, I thought they only dragged themselves along." No, there is no dragging about an alligator. The instant danger is detected he raises his body a foot or more from the ground and rushes off like a gigantic lizard, arching his back and bounding along at a speed almost incredible in so ungainly a creature. The instant he touches soft mud his immense tail acts as an auxiliary propeller, swishing from side to side with terrific power, and as soon as he enters the water the tail alone does duty, sending him plowing along at a speed no one could believe who had seen these creatures loafing in tanks in zoological gardens. From what I afterward saw of "muggers," as they call them in India, I am convinced a blow from a large one's tail would not only break a man's legs, but absolutely reduce him to pulp.

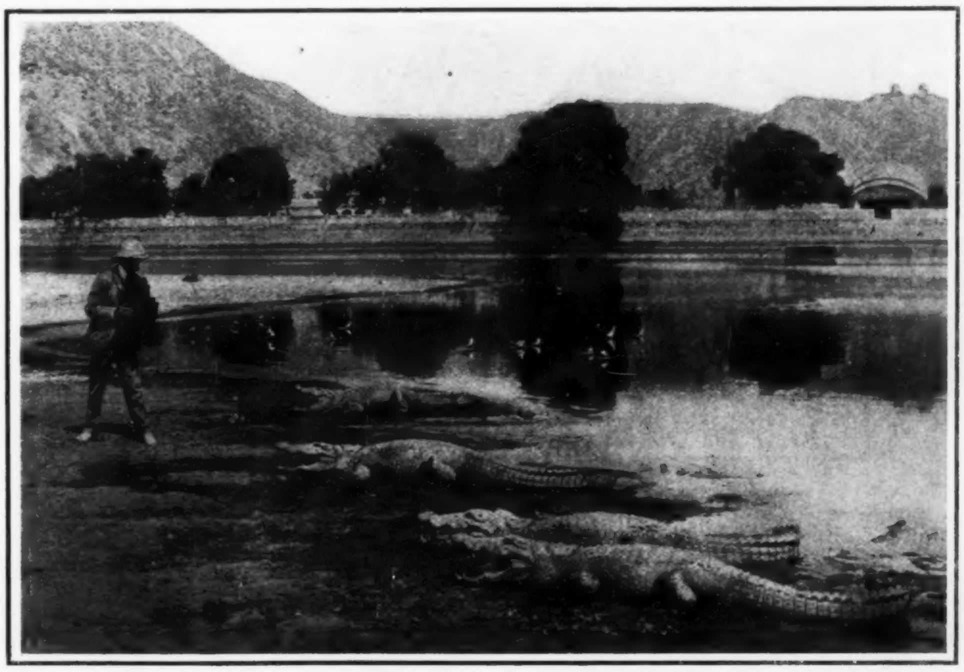
On arrival at every likely place in India I inquired about these creatures, and at Calcutta I learned that a leather company had spent a large sum in endeavoring to secure a good photograph of an alligator in his wild state, and had been unsuccessful. My prospects of achieving my ambition did not therefore seem very hopeful. In due course I arrived at a certain town where I heard that in a lake a large number of muggers were kept, free and unconfined, at the pleasure of the maharajah. I did not waste any time in securing permission to visit this place, as here, if anywhere, I should, I hoped, get what I so much desired. On arriving at the lake, or "tank," as a lake is always called in India regardless of its size, my first feeling was of disappointment. It was, perhaps, an eighth of a mile square and surrounded by steep walls, but the long-continued drought had so reduced the quantity of water it contained that it presented little of the beauty which I had heard it possessed. It seemed little else but hard-baked mud. On glancing around, however, I saw several of the stranded logs, which I knew would immediately come to life if anyone went near. My rajput guide intimated that we were to descend the steps and approach them. I must say I felt a bit chary about this, as, knowing the speed at which they could run, I hesitated to expose myself on that waste of mud with no retreat in reach, should they be disposed to attack, instead of run away. The swarthy rajput, however, assured me we might approach reasonably near, and several others of his comrades appearing on the scene, we all went down together. These men were those whose duty it was to see that a certain prescribed quantity of offal and meat was cast into the lake each day to feed the brutes. As the men went near the muggers exhibited no desire to molest them, and, emboldened by their apparent tameness, I approached also with my camera, but was instantly met with a chorus of savage snorts,



A NICELY-POSED GROUP OF FIVE.



AN INDIAN "TANK."



THE PHOTOGRAPHER AT CLOSE QUARTERS.

PHOTOGRAPHING ALLIGATORS.

* Country Life (London).

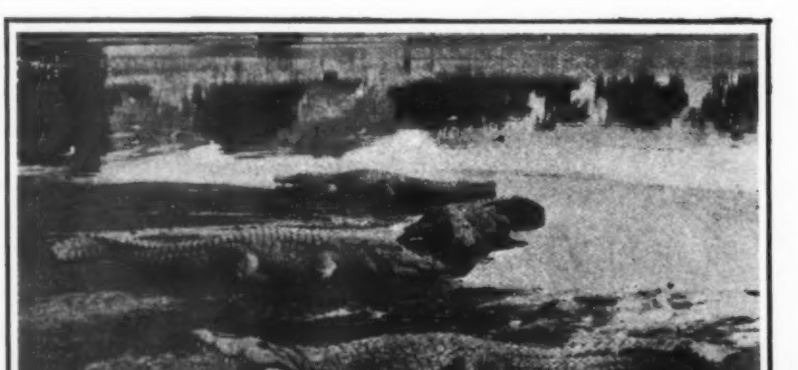
which convinced me I had better remain at a safer distance. The alligators seemed to have become alarmed, however, for they all ran into the water and retired from view.

How to get them out again? Ah! that would be easy. We had only to buy meat, plenty of meat, my rajput said. "Then go and buy meat," I replied. "Buy plenty of it; buy half a cow if necessary, for we must get them out again." And off went two of the men, and returned in half an hour with four huge baskets of butcher's scraps. One of the stiff-whiskered, fierce-looking rajputs then went to the water's edge and uttered a long and shrill yell. No sign of life appearing to answer this summons, he yelled again and again, making the place ring with his cries. Presently an eddy appeared on the surface, then an ugly snout which slowly approached until the owner of it stranded in the mud. As he evinced no desire to come any nearer some scraps were thrown to him, which he seemed to greatly relish. Then one of the men tied a large piece of meat on to a string and threw it in front of him. As he darted at this it was drawn away from him and then cast again until he had been tempted well up on to the dry ground.

Others now began to appear on the surface and were lured ashore by similar means. I was busy photographing each specimen as he emerged from the water. By means of a pair of 8-inch lenses on a stereoscopic camera I was able to keep at a safe distance and yet secure fair-sized portraits of them. They were easily alarmed by any sudden movement and would rush off into safety again; but the tasty scraps were irresistible. Like Oliver Twist they asked for more. It was not easy, however, to get the brutes on to the high and dry ground. They preferred to remain on the safe side of the water's edge, where their powerful tails could help them maneuver even more rapidly than was possible on the hard surface. There was no running or hurrying about them unless alarmed. They appeared to be most sluggish brutes; but the instant they were frightened at anything they would swing round and rush back to safety with the speed of which I have already spoken.

Having now secured a good selection of stereographs, I essayed to get a large picture with my tripod camera. This I successfully accomplished by the use of a 14-inch Zeiss protar, and then becoming more ambitious I decided to attempt some groups. This was very difficult, for as fast as two or three had been coaxed out they would be frightened at our efforts to add to the number, and we had repeatedly to begin over again. My groups on the day of my first visit were so successful, however, and I was so pleased with them, that I decided to try to get even better ones. So the next day found me there again. On this occasion, after several hours' work, I had the pleasure of securing a nicely posed group of five. Posing alligators! It does sound a bit absurd, doesn't it? But that is exactly what we did. By means of meat tied to long strings, and hung on long bamboo poles, they were coaxed, inch by inch, into the very positions I desired, and the groups herewith are the results. While I was busy making stereographs of my group my friend, Cecil H. Meares, who was using my tripod camera, took the photograph showing me at work on them.

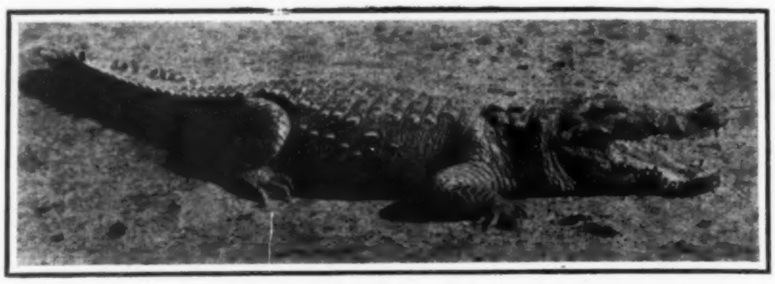
The Indians repeatedly warned me not to approach



AT THE WATER'S EDGE.

PHOTOGRAPHING ALLIGATORS.

too near; but thinking the muggers were as much afraid of me as I of them, I became foolishly bold, and in the end had nearly cause to rue my enthusiasm. I was intently centering one on the ground-glass, with my eyes well down into the hood of my reflex, when I thoughtlessly stepped back a few paces, quite forgetting that there was another ten-footer close behind me. Suddenly there was a fearful snort, the Indians yelled, there was a patter of feet, and without turning to look I took a leap and then ran. I was not the fraction of a second too soon, for the brute's jaws came together with a loud snap that fairly made my blood chill, as I realized that only my leap had saved me from being badly mangled, or, as would more probably have been the case, set upon by the lot of them and dragged into the lake.



A TWELVE-FOOTER.

However, I had achieved my ambition, and had got the pictures I desired. I had, indeed, got far more than I ever expected; and if they were not exactly wild alligators, they were certainly savage enough on provocation, as I had nearly proved to my cost.

A STORY OF WHEWELL AND HAMILTON.*

By PROF. CASSIUS J. KEYSER.

IN 1835 William Whewell, then fellow and tutor of Trinity College, Cambridge, published an appreciative pamphlet entitled "Thoughts on the Study of Mathematics as a Part of a Liberal Education." The author was a brilliant scholar. "Science was his forte," but "omniscience his foible," and his reputation for universal knowledge was looming large. That reputation, however, Sir William Hamilton regarded as his own prerogative. None might dispute the claim, much less share the glory of having it acknowledged on his own behalf. Whewell must be crushed. In the following year Sir William replies in the Edinburgh Review, and such a show of learning! The reader is apparently confronted with the assembled opinions of the learned world, and—what is more amazing—they all agree. Literati of every kind, of all nations and every tongue, orators, philosophers, educators, scientific men, ancient and modern, known and unknown, all are made to support Hamilton's claim, and even the most celebrated mathematicians seem eager to declare that the study of mathematics is unworthy of genius and injures the mind. Whewell was overwhelmed, reduced to silence. His promised rejoinder failed to appear. The Scotchman's victory was complete, his fame enhanced, and his alleged judgment regarding a great human interest of which he was ignorant has reigned over the minds of thousands of men who have been either willing or constrained to depend on borrowed estimates. But even all this may be condoned. Jealousy, vanity, parade of learning, may be pardoned even in a philosopher. Hamilton's deadly sin was none of these, it was sinning against the light. In October, 1877, A. T. Bledsoe, then editor of the Southern Review—unfortunately too little known—published an article in that journal in which he proved beyond a reasonable doubt—I have been at the pains to verify the proof—that Hamilton by studied selections and omissions deliberately and maliciously misrepresented the great authors from whom he quoted—d'Alembert, Blaise Pascal, Descartes, and others—distorting their express and unmistakable meaning even to the extent of complete inversion. This same verdict regarding Hamilton's vandalism, in so far as it relates to the works of Descartes, was independently reached by Prof. Pringsheim, and in 1904 announced by him in his Festrede before the Munich Academy of Sciences. As for Schopenhauer, I regret to say that a similar charge and finding stand against him also. For not only did he indorse without examination and re-utter Hamilton's tirade in the strongest terms, thus reinforcing it and giving it currency on the Continent, but, as Pringsheim has shown, the German philosopher, by careful excision from the writings of Lichtenberg, converts that distinguished physicist's just strictures on the then flourishing but wayward Combinatorial School of mathematics into a severe condemnation of mathematicians in general, and of the science itself, which, nevertheless, in the opening but omitted line of the very passage from which Schopenhauer quotes, is characterized by Lichtenberg as "eine gar herrliche Wissenschaft."

* Abstract from a lecture on mathematics, delivered at Columbia University, in the series on Science, Philosophy and Art.

IMPORTANT REPORT ON GEOLOGICAL CHEMISTRY.

A NOTABLE work is now in press for publication by the United States Geological Survey—a bulletin entitled "The Data of Geochemistry," No. 330 of the Survey's series, by Mr. F. W. Clarke, the chief chemist. The various chemical changes taking place in the crust of the earth, with its liquid and gaseous envelopes, the ocean and the atmosphere, are the field of investigation of the geological chemist. From a geological point of view the solid crust of the earth is the chief object of his study, and the reactions which take place in it he classes, for convenience, under three heads: First, reactions between the essential constituents of the crust itself; second, reactions due to the aqueous envelope; and third, reactions produced by the atmosphere. That the three classes are not sharply defined but grade into one another is admitted, but the distinction between them is valid enough to serve a good purpose in the arrangement and discussion of the data. Furthermore, for convenience of study, the solid crust of the earth may be regarded as made up of three layers or shells, which interpenetrate one another to some extent, but which are nevertheless definite enough to consider separately. The innermost is a shell of crystalline rocks, of unknown thickness, which forms the nearest approach to the original material of which the crust was composed; next overlying this is a shell of sedimentary or fragmental rocks; and above this is the third layer of soils, clays, and gravels. The second and third layers are relatively thin and are derived chiefly from the first in great part through the transforming agency of waters and of the atmosphere, although organic life has had some share in bringing about certain of the changes. It is the history of these innumerable changes which Prof. Clarke has presented in his report.

Upon the subject of geochemistry a vast literature exists, but it is widely scattered and portions of it are difficult of access. The great general treatises are not recent, and enormous masses of modern data are as yet uncorrelated. The American material alone is singularly rich, but most of it has been accumulated since the publication of the classical works of Roth and Bischof. The science of chemistry, moreover, has undergone great changes during the last twenty-five years, and many subjects now appear under new and generally unfamiliar aspects. To bring some of the data together, to formulate a few of the problems, and to present certain of the conclusions in their modern form, are the purposes which Prof. Clarke had before him in the preparation of his memoir. "It is not," he says, "an exhaustive monograph upon geochemistry, but rather a critical summary of what is now known and a guide to the more important literature to the subject. If it does no more than to make existing data available to the reader, its preparation will be justified."

ATMOSPHERIC NITROGEN.—II.*

ITS FIXATION IN AMERICA.

BY GEORGE M. HEATH.

Concluded from Supplement No. 1691, page 339.

ABSORPTION OF NITRIC OXIDE.

So far, only the oxidation of the nitrogen has been discussed. The great rarefaction of the gases to be worked with greatly enhances the difficulties of the problem. The gases that come from the furnace with a temperature of about 650 deg. C. and containing about 2 per cent of nitric oxide, first pass through specially constructed evaporating pans, where the waste heat is utilized for the evaporation of solutions of the nitrate salts to be manufactured. The cooled gases then pass into an oxidation chamber, where the nitric oxide combines with more oxygen to form nitrogen dioxide. As three volumes of oxygen are required for the complete oxidation of four volumes of nitric oxide, and as the exit gas from the furnace always contains a much greater proportion of oxygen, no addition is necessary. A complete oxidation of the nitric oxide to nitrogen dioxide does not take place instantaneously, but requires a comparatively long time. From the oxidation chambers the gases pass into several towers sprayed with water or weak acid which is being brought up to the desired strength. From these towers, the gases, which still contain small amounts of nitric oxide, go to other towers containing limestone and sprayed with milk of lime, and finally emerge deprived of all oxides of nitrogen. By this process an acid of 5 per cent strength can be obtained. If the gases are absorbed in water heated to 60 deg. C., it is possible to obtain an acid of 60 per cent concentration and free from nitrous acid. Theoretical considerations indicate that an acid containing 63.62 per cent of actual nitric acid and corresponding to the formula HNO_3—2H2O is the strongest acid that can be obtained directly from oxidation and absorption of nitric oxide.

COST OF MANUFACTURE, HEATH PROCESS.

In estimating the cost of fixed nitrogen by the Heath process, the item of electric power is of greatest importance. The following estimated cost is based on the cost of Niagara Falls power, namely, $26.70 per kilowatt year, and the production of a 50 per cent acid.

	Per 1200 kilos actual HNO_3
Electric power, one kilowatt year at $26.70	$26.70
Labor	6.50
Repairs	2.00
General expenses	3.00
Interest and depreciation, 10 per cent of $50	5.00
	$43.20

Equivalent to 1.64 cents per pound of actual nitric acid, or 7.3 cents per pound of fixed nitrogen.

The cost of strong nitric acid made from nitrate of soda during 1906 was about five cents per pound of actual nitric acid. Of this figure, the cost of concentrating from the 50 per cent acid to a 96 per cent acid was about 2 cents per pound. The chief item of cost in the usual process of concentration is the reconcentration of the sulphuric acid used. What seems to be more hopeful is the concentration of the 30 or 50 per cent acid by electrolysis. I find that by this method a 30 or 50 per cent acid can be concentrated to a 96 per cent acid at a cost of about a third of the present cost. To obtain 100 kilos of a 96 per cent acid, 350 kilowatt hours of electrical energy are consumed, and there is no loss of raw material. This corresponds to 2,503 kilos of 96 per cent acid per kilowatt year.

With electrical energy at $26.70 per kilowatt year, the cost of concentrating would be $10.68 per 1,000 kilos, or 0.49 cent per pound.

If the weak nitric acid is converted on the spot into basic calcium nitrate, containing 13.5 per cent of nitrogen, it would make 1,000 kilos of this product cost $28, as against $49 for 1,000 kilos of Chilian nitrate.

The manufacture of ammonium nitrate furnishes another large field for the use of synthetic nitric acid. During 1907 one firm used over 11,000,000 pounds at a cost of 6.3 cents per pound. By using synthetic nitric acid it can be made for 4.5 cents per pound, leaving a large margin of profit.

Many more uses might be given to show the commercial importance of this problem; but before I close, I wish to draw attention to the production and cost of power, which is the most important factor in the success of the problem. Here, as in other more familiar cases, the law of supply and demand will hold good. Water power is rapidly developing in all parts of the United States. The development of power on the Illinois River, the McCall's Ferry Company, the Chattanooga and Tennessee River Power Company, the Whitney Power Company, and many other companies, whose plants are being completed, will furnish thousands of horse-power as cheap as that obtainable at Niagara Falls; and through their more central locations, will greatly aid in the success of this problem, where the production of artificial fertilizers is to be carried on. Attention might be called to the economical production of electrical energy from blast furnace gases and the Mond producer-gas system. The blast furnace gases which are regularly produced in the manufacture of iron in the United States, would yield an annual production of about 1,250,000 horse-power. Assuming a production of one ton of sodium nitrate per 1 horse-power year, would make an annual production of 1,250,000 tons of sodium nitrate. Thus, you see, the blast furnaces in the United States could supply an amount nearly equal to the present world's consumption. Using the Mond producer-gas system, where combustibles of inferior quality can be used, and utilizing the by-product, ammonium sulphate, the cost of electrical energy with coal at $3 per ton would amount to about $14 per horse-power year.

It is highly probable that considerable of the power from blast furnace gases will be employed in the iron and steel industry, as is the case in Gary, Ind., where over 100,000 horse-power is to be generated. Nevertheless a large surplus will remain, which will be available as a source of cheaper power than that obtainable from any other source in this country, even that derived from most waterfalls, the cheapness of which, especially in foreign countries, is greatly overstimated.

OTHER COMPANIES OPERATING.

As illustrating the extent to which the importance of this subject is recognized in Europe, the following list is given of companies that have been formed to manufacture nitrogenous products from the atmosphere:

In December, 1905, the Norwegian Hydro-Electric Nitrogen Company, Ltd., of Christiania, Norway, was formed with a capital of $1,876,000 for operating the Birkeland-Eyde patents in Norway, and for manufacturing nitric acid and nitrates on a commercial scale. The capital for the undertaking was largely furnished by banks in Paris, Berlin, and Dresden, and in Norway, Sweden, Denmark, and Russia. In December, 1906, the capitalization was increased to $5,360,000, one-half of the new issue, or $1,742,000, being subscribed by the Badische Anilin und Soda Fabrik, of Mannheim, Germany.

Four waterfalls, capable of furnishing an aggregate of nearly 300,000 horse-power, in the neighborhood of Notodden, Norway, were secured at very little cost, one fall of 30,000 horse-power costing only $8,000, and another of 200,000 horse-power costing $24,000. Only one water-power is being developed at present, and this is estimated to furnish about 30,000 horse-power. Operations on a small scale were begun at Notodden in May, 1905, using power purchased from a neighboring power plant, but the factory has not been continuously in operation.

A new factory, to be located alongside the old one, was projected, which was to contain 32 furnaces, and utilize 26,000 horse-power, or about 20,000 kilowatts. This factory was to be in operation by the end of 1907.

Societa Generale Elettrica del Adamelle, of Milan, Italy. Capital $1,930,000, subscribed by Italian, French, and Swiss bankers.

"Dynamo," of Milan, Italy. Capital $1,930,000, subscribed by Italian, French, and Swiss bankers.

"Azote," of Geneva, Switzerland. Capital $386,000, subscribed by the Metallurgische Gesellschaft, the Frankfurt-am-Main, the Consolidated Alkali Works of Westergelsa, and other large electro-chemical companies. S. Singer of the Dynamite Nobel, of Paris, is a member of the board of directors.

The International Nitrin Company, of Zurich, Switzerland. Capital, $3,860,000. Expects to construct works on the Malaja Pass, Switzerland.

The Initiative Committee for the Manufacture of Nitrogenous Products, of Freiburg, Switzerland, under the direction of Kowalski and Moscicki, is investigating the problem, but the amount of capital back of the organization is not stated.

In addition it is officially stated that the Badische Anilin und Soda Fabrik, the Berliner Anilin Fabrikation A. G., and the Elberfeld Farben Fabrik, acting as a cartel, have subscribed one-half of the capital stock of each of two Norwegian companies for the fixation of atmospheric nitrogen; the total capital of the two companies being $9,112,000. One of the companies referred to is the Norwegian Hydro-Electric Nitrogen Company; and the other, with capital of $3,752,000, is to operate the Frank-Caro process.

The Badische Anilin und Soda Fabrik is also said to be preparing to develop a water power on the river Alz in southeastern Bavaria near the Austrian frontier, where 50,000 horse-power will be used. Among other products they propose to manufacture nitrate of potash for the explosives industry. A head of 165 feet is to be utilized, and the amount of the investment is placed at $7,500,000.

It is rare to find a chemical process so simple; the only expenses to be considered are, on the one hand, electric power, and, on the other hand, current manufacturing expenses. Finally, an occurrence practically unique in the industrial world—the cost of the raw material is zero, if it is simply a question of preparing nitric acid, and is almost negligible when nitrate of lime is produced.

* Read before the Philadelphia Section of the American Chemical Society.

QUALITY OF WATER IN THE POTOMAC RIVER.

The Potomac River carries past the town of Cumberland, Md., each year, 25,000 tons of mineral matter held in suspension and 120,600 tons of dissolved matter, of which probably 40,000 tons are derived from coal mines, according to Mr. R. B. Dole, of the United States Geological Survey. That is, each acre of the drainage basin above Cumberland contributes to the river 2.04 pounds of mineral matter, of which 0.36 pound is suspended and 1.68 pounds dissolved.

The dissolved mineral matter in the water is composed as follows: Silica, 7.1 per cent; iron, 0.11 per cent; calcium, 18.6 per cent; magnesium, 3.6 per cent; sodium and potassium, 7.0 per cent; carbonate and bicarbonate radicles, 13.5 per cent; sulphate radicle, 45.7 per cent; and chlorine, 4.3 per cent. The effect of the mine drainage reaching the river is shown by the large proportion of sulphates. The average quantity of suspended matter carried by the water is about 25 parts per million, but this is subject to much variation, the extreme being about 5.6 times the average. The dissolved mineral matter ranges between 66 and 233 parts per million, making an extreme variation of 1.4 times the average.

Although probably good for industrial use when uncontaminated, the water of the Potomac at Cumberland will now form a hard scale in boilers and is less desirable for use in many industries than the normal streams of near-by areas.

The investigation of the quality of Potomac River water at Cumberland has been carried on by the Survey for a year, being part of a systematic study of the quality of water carried by the principal rivers of the country. In the course of this study samples were collected each day at stations established on the streams and forwarded to the laboratories of the Survey, where they were combined in sets of ten to form composites which would represent the average conditions of the streams for ten consecutive days. Complete mineral analyses of these composite samples furnish reliable data as to the average quality and variations in the quality of the stream water, and these data are used as bases for calculating the amount of chemical and mechanical denudation effected by our rivers and for estimating the value of the water for industrial and other purposes. The results of the work will be published by the Survey in a number of water-supply papers now in preparation.

YOUR HORSE'S FEET.

A horse should never be compelled to stand uphill. The anatomy of the horse's foot, and, indeed, the shape of the horse himself, makes this an uncomfortable and unrestful position.

Whatever the arrangement for drainage is, the horse must stand as nearly level as possible. Moreover, he must stand upon a dry surface unless it is found that his feet need moisture. In such cases a wet clay floor is excellent, temporarily.

When you come in from driving, and after your horse has properly cooled off, see that the mud is removed, not only from his legs and outer portions of his hoofs, but from the sole as well. An occasional stuffing with flaxseed is not only beneficial but necessary.—Suburban Life.

THE TREATMENT OF TIMBER.*

THE OPEN-TANK METHOD.

BY CARL G. CRAWFORD.

WHEN the preservative treatment of timber was first introduced into this country, the lumber situation was wholly different from what it is now. There was a seemingly exhaustless supply of structural timber of the finest grades, and hence the preservative treatment of timber for different uses was economical only in the comparatively few cases where the cost of renewals was unusually high. As the supply of timber gradually diminished under the enormous inroads made upon our forest resources by the industrial development of the country, the prices of the better grades of timber doubled and trebled, until it is now difficult in many places to obtain regularly an adequate supply at any price. The increased cost of material has made it necessary to use less durable but more plentiful timbers, which require some form of artificial treatment before they can be expected to give as long service as the more durable woods. Hence the amount of timber subjected to artificial preservation has greatly increased, and the number and capacity of treating plants throughout the country have increased accordingly.

Most of the processes in general use require elaborate and expensive plants, consisting of closed retorts capable of withstanding high pressures, of vacuum and pressure pumps, steam boilers, etc. Such plants are usually stationary. The high cost of erecting them requires that they be located where the supply of timber will be continuous. This frequently means that timber must be transported long distances and double freight charges paid. Moreover, the interest on the investment and the cost of operating make it impracticable in most cases to use such plants for the treatment of timbers which are required in large quantities and at a comparatively low cost.

There has thus grown up a demand for some cheap and simple process of wood preservation adapted for timber in common use, for which pressure methods are too expensive. The general adoption of such a process would largely increase the amount of timber artificially preserved, and so result in a great saving in the amount of timber consumed annually in the United States. It would insure the more complete utilization of the forest products, and encourage the use of many so-called "inferior," or quickly decaying, woods for purposes for which only high-grade, or decay-resistant, woods are now almost exclusively used.

HISTORY OF THE OPEN-TANK METHOD.

There have been many attempts to find a cheap and effective preservative process requiring no costly mechanical equipment. Most of these attempts were founded on an unsound theoretical basis, and many of them failed for other reasons. Obviously one of the simplest methods of securing a penetration is to boil the wood in the preservative, and this method has been frequently employed. It was not, however, until the year 1867, when Prof. Charles A. Seely, of New York, discovered and patented the process which bore his name, that the true value of the boiling method was ascertained. Through Seely's attempt to increase the penetration by immersing the timber in a bath of coal preservative after it had remained for some time in the boiling liquid was discovered the principle which later developed into the open-tank method of to-day. Apparently, however, Seely's investigations attracted but little attention at the time, and it appears to have been practically abandoned after a few years.

At the Louisiana Purchase Exposition a series of experiments was undertaken by the Forest Service to determine some practical inexpensive method of prolonging the life of fence posts. In the St. Louis experiments, boiling the posts in a vat containing tar oil was at first attempted, with only fair results, but in the course of the tests to increase the penetration the principle of the method once employed by Seely was revived. Subsequent developments in the experiments in which the Forest Service has applied this principle have resulted in what is now known as the open-tank method.

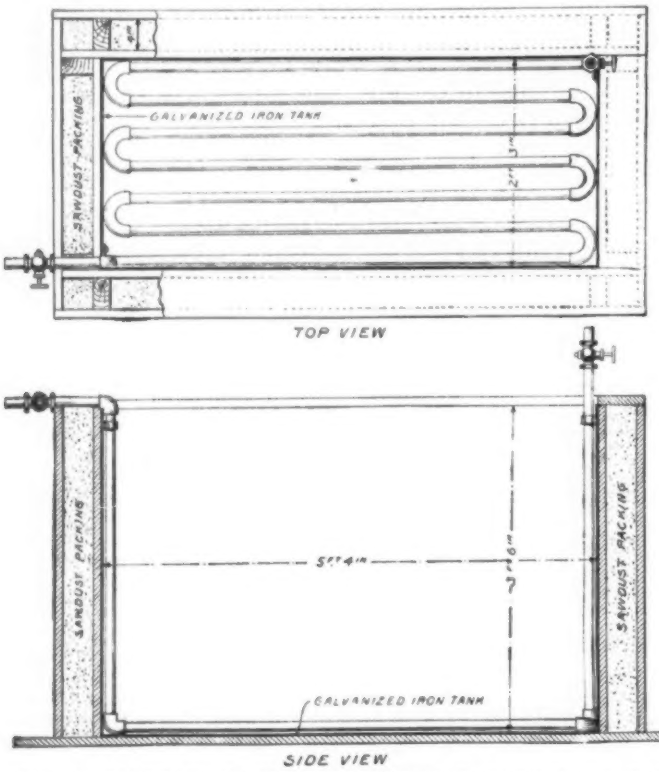


FIG. 1.—DIAGRAM OF AN EXPERIMENTAL TANK USED FOR TREATING FENCE POSTS.

As already suggested, the open-tank method is based upon the use of an open tank, capable of withstanding heat, and either equipped with steam coils or so arranged that fire can be placed underneath.

Sufficient preservative is run into the tank to cover the portion of the timber which is to be treated, and the temperature of the liquid is then raised slightly above the boiling point of water. This temperature is maintained for a length of time depending upon the character of the wood and the treatment desired. At the end of the hot bath the timber is either quickly transferred to another vat, containing a cold preservative, in which it is submerged for a definite period of time, or else the heat is shut off and the timber is allowed to remain in the cooling liquid until the required absorption is obtained or until no further absorption takes place. The time required by the treatment may be shortened, without transferring the timber from one vat to another, by running out the hot liquid at the end of the hot bath and simultaneously letting in the cold liquid.

The former theory of the process held that the hot preservative converted the moisture in the wood into steam and so expelled a large percentage of it, and that the cold bath condensed the steam in the pores of the wood and so formed a vacuum into which the preservative was forced by atmospheric pressure. In this way it was thought that the timber became seasoned just before or simultaneously with the entrance of the preservative. From this theory it also followed that green timber containing a large amount of moisture could be given a better treatment than seasoned timber, since the greater the amount of moisture in the wood the greater would be the vacuum formed when the vapor is driven off.

Recent experiments of the Forest Service with the open-tank process and with other processes have brought out, among other things, that though fairly good results were obtained by the open-tank method as formerly carried on, the theory itself was incorrect. The amount of space occupied by air in the timber was given too little consideration. It is true that a vacuum is created in the cellular structure of the timber, to destroy which the preservative is forced into the wood in part by atmospheric pressure. This vacuum, however, is principally produced not by the vaporization of the water, as was formerly supposed, but by the expansion and expulsion of the air within the wood cells and intercellular spaces. Wood, as is well known, is a very poor conductor of heat. It is almost

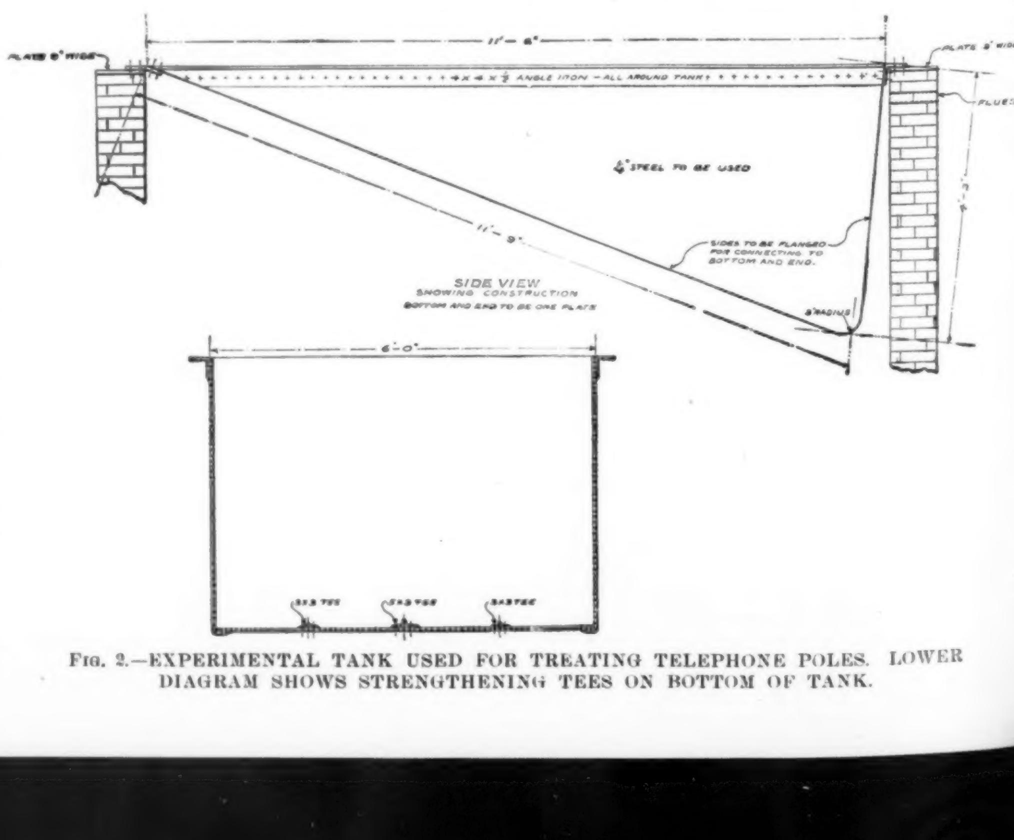


FIG. 2.—EXPERIMENTAL TANK USED FOR TREATING TELEPHONE POLES. LOWER DIAGRAM SHOWS STRENGTHENING TEES ON BOTTOM OF TANK.

* Abstracted from a circular issued by the United States Department of Agriculture.

impossible, therefore, in any reasonable time to heat a piece of timber to or above the boiling point of water, except in a thin exterior layer, without employing temperatures which would injure the fiber and volatilize large quantities of the preservative. Hence it is unreasonable to suppose that any considerable quantity of the water in the interior of the wood is sufficiently heated by the open-tank method to volatilize and escape.

If, again, as was formerly held, the creation of the vacuum were due wholly to the volatilization of the moisture in the wood structure, it naturally would follow that green wood, which contains relatively large quantities of water, would secure a stronger vacuum and therefore a better penetration than would seasoned wood from which most of the moisture had already evaporated. But numerous experiments carried on by the Forest Service have conclusively shown that, no matter what the process of treatment, a deeper and more uniform penetration will be secured if the timber is first seasoned, so that any theory which appears to explain why green timber is better adapted to treatment than is seasoned timber must be founded partly on error.

It is a familiar law of physics that at the boiling point of water it expands much more than air does, whereas at lower temperatures the reverse is true. Since, in practice, the wood can not be heated throughout by the open-tank method to the temperature of boiling water, the lower temperatures which are actually maintained must naturally have a greater effect on the air within the cellular structure than on the water.

In consideration of these and other observations it is believed that the creation of the vacuum is due in a larger degree to the expansion and driving off of the air than to the vaporization and driving off of the moisture; that while the latter assists in producing the vacuum it is not so important as was formerly held, and a good vacuum can be secured without it.

Capillary attraction undoubtedly assists in the penetration of the preservative, though it is of secondary importance, especially where the absorption is at right angles to the grain of the wood. It is obviously greater in seasoned wood than where the intercellular spaces are obstructed by sap.

In the treatment of any timber by any process, the added life is directly proportional to the amount of absorption and the depth of the penetration secured. But where an expensive preservative is used, or where only a limited service is desired, it is often necessary to limit the absorption of the preservative and so, in a measure, lessen the penetration. It was in the effort to determine some method by which the amount of absorption and penetration of the preservative could be controlled with a fair degree of accuracy that the true respective values of hot and cold baths were demonstrated.

It is now established that the hot bath should be continued long enough to heat the timber and expand the air through a deeper zone than it is desired to penetrate with the preservative; and that the depth of the penetration should be regulated by the cold bath rather than by prolonging or shortening the hot bath and allowing the timbers to remain in the cold preservative for an indefinite time. A thorough heating gives a much better and more even penetration, by expanding the openings into the cellular spaces and allowing freer circulation through the pores of the timber. Moreover, when the outer zone has been thoroughly heated the timber may be removed from the cold preservative before the vacuum has been completely destroyed in the interior of the wood, and in this way a better penetration is secured with a less amount of the preservative, since the liquid in the outer saturated zone is drawn in after the timber is removed, leaving the surface clean and dry. It has frequently been shown that very little penetration is obtained in the hot bath, and that almost the entire penetration is secured during the cold or cooling bath. This holds in all cases unless the timber is thoroughly seasoned and absorbs the preservative with exceptional readiness.

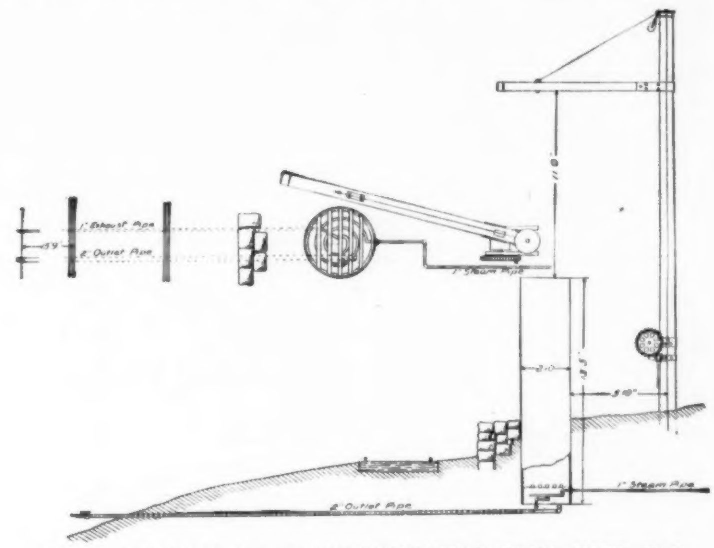


FIG. 3.—DIAGRAM OF AN EXPERIMENTAL TANK USED FOR TREATING MINE TIMBERS.

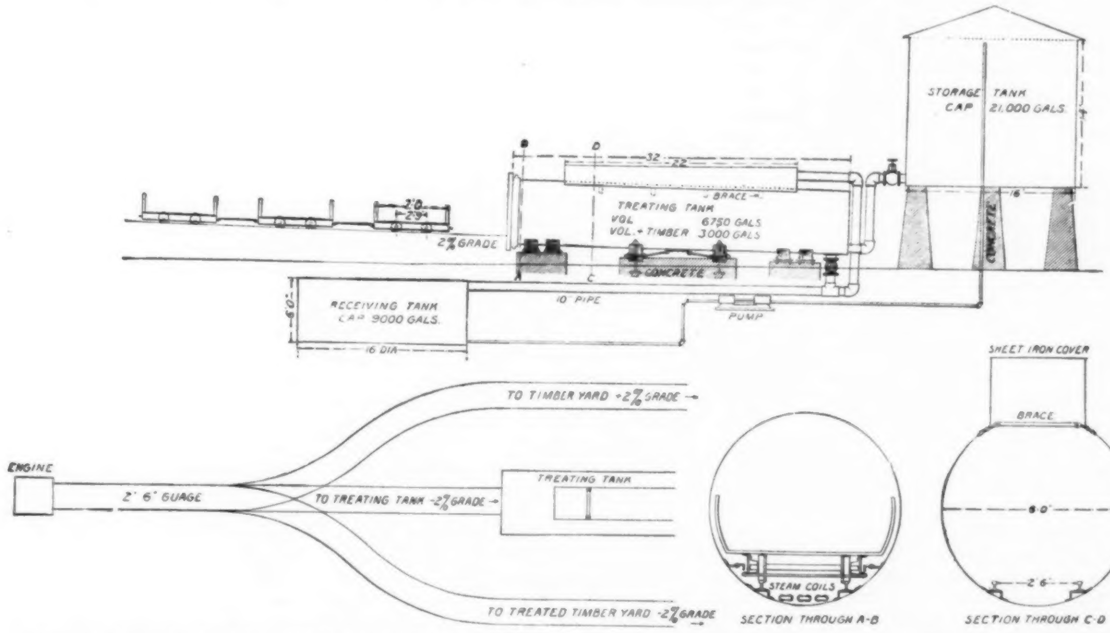


FIG. 4.—DIAGRAM OF A SMALL COMMERCIAL PLANT FOR TREATING MINE TIMBERS, CROSS-TIES, CROSS-ARMS, ETC.

The temperature used in the open-tank treatment must depend on the nature of the preservative and the condition of the timber. If water solutions are employed, the hot bath should be carefully kept at the boiling point. If the temperature is allowed to fall intermittently during the treatment, the vacuum in the timber will be partially destroyed by the entrance of the preservative, which will obstruct the escape of the air when the temperature again rises. With complex and expensive preservatives, such as creosote oil, the temperature should be kept as low as is consistent with securing the desired penetration, since the loss by volatilization during the treatment is almost directly proportional to the temperature and duration of the hot bath. For this reason the tank should be so constructed that a minimum amount of oil surface is exposed to the air.

In treatment by the pressure-cylinder processes, especially if the timber has already reached a more or less air-dry condition, care must be exercised to prevent the temperature from rising to a point injurious to the wood fiber. With the open-tank method, however, at least with the preservatives in use at the present time, there is much less likelihood of injury from this source. In the case of salt solutions, the boiling points do not greatly exceed that of water; in the case of oils, their volatility, even at temperatures far below the boiling points of any of their constituents, renders it necessary to keep the temperature at the lowest practicable point if the most economic treatment is to be secured. The treatment of green timbers and timbers which do not absorb the preservative readily requires the use of higher temperature through longer periods.

In selecting creosote oil for all open-tank treatments, in order that the loss by volatilization may be reduced to a minimum the preference should be given to oils which contain the largest percentages of constituents with high boiling points. This is especially true if the character of the timber calls for an unusually severe treatment. In general it may be said that for green timber the temperature should not exceed 230 deg. F. nor fall below the boiling point of water. For seasoned timber the temperature should not be allowed to exceed the boiling point of water by more than 8 or 10 degrees.

It has already been shown that the duration of the cold bath depends upon the penetration which it is desired to secure. Porous timbers of small dimensions may be saturated after a comparatively short immersion in the cold bath, while for larger sizes, and for timbers which do not absorb the preservative readily, several hours are necessary. The duration of the hot bath will depend upon the size of the timber, its moisture content, and the ease with which it absorbs the preservative. In general, however, it may be said that the maximum penetration for green timber can be secured by a hot bath of from eight to ten hours followed by a cold bath of from eight to sixteen hours; whereas seasoned timber will require only from three to six hours in the hot preservative, followed by a sufficiently long immersion in the cold bath to secure the desired penetration, probably in no case exceeding eight hours.

When the timbers are transferred from hot to cold oil they should be exposed to the air no longer than is absolutely necessary. In treating small timbers, which are easily and quickly handled, it is better to change the timbers from hot to cold oil than to permit the oil to cool or change the oil in the treating tank.

Any of the preservatives in general use can be

applied by the open-tank method, provided that the temperatures are properly controlled. Oils with high boiling points are applied with less difficulty and less loss by evaporation than those with low boiling points. In applying preservatives held in water solution some of the water is evaporated during the treatment, with a consequent strengthening of the solution; but treatment can easily be regulated either by the addition of hot water during treatment, or, better, by using a solution slightly weaker than desired for impregnation.

The open-tank method is applicable to the treatment of fence posts, telephone poles, mine props, small dimension timbers, cross-ties, piling, and similar timbers. Figs. 1, 2, and 3 are diagrams of experimental tanks used for the treatment of fence posts, telephone poles, and mine timbers. Fig. 4 is a diagram of a small commercial plant for the treatment of mine timbers, cross-ties, or cross-arms. The best results are obtained in the treatment of round timbers on which an unbroken zone of porous sapwood surrounds the more impervious heartwood. For this reason round fence posts are preferable for treatment to split posts, in which the heartwood is directly exposed. One of the chief advantages of the process lies in the fact that it can be effectively applied to parts of timbers which are especially subjected to rapid decay, such as the butts of fence posts and telephone poles, without wasting preservative on other parts.

Extensive experiments by the Forest Service with fence posts, telephone poles, and mine timbers have given satisfactory results. There is little doubt that the method is applicable to the treatment of small-dimension timbers, such as cross-arms, underground conduits, shingles, and other wood in small sizes. No difficulty should be encountered in treating cross-ties and piling of such woods as loblolly pine, black and tupelo gum, western yellow pine, and lodgepole pine. Fairly good results have been obtained in the treatment of arborvitæ, chestnut, and red oak, but the experiments with these woods do not yet warrant the application of the method to the treatment of piling and ties manufactured from them. The process is being rapidly developed, and it is probable that future experiments will reveal methods of applying it to many other species and purposes.

Though considerable progress has been made within the last few years in our knowledge of the ease or difficulty with which different timbers absorb the preservative under varying conditions, much yet remains to be done. It is therefore unwise at the present time to predict in detail the possibilities of the open-tank method. It is certain, however, that for many forms of timber manufactured from many species, this method is practicable in cases where no other process could be employed, and that its general application will make possible the treatment of much larger quantities of timber in the future than in the past.

For those cases to which it is properly applicable, the saving effected by the open-tank method is readily apparent. Not only is the cost of operation and maintenance comparatively slight, but the cost of installation is probably less than one-fourth of that of a pressure-cylinder plant of the same daily capacity. Moreover, the open-tank apparatus may be moved with comparative ease, so that it may more nearly follow the base of supply, with a consequent saving of freight charges. It should be distinctly borne in mind that it is not a universal substitute for the pressure processes, and its future usefulness will largely depend upon the care and foresight which are exercised in its application.

The simplest type of open-tank apparatus consists of a vat to be partly filled with a preservative, in which the timber is submerged, and under which a fire can be built. An apparatus of this kind may be placed on wheels and drawn from one place to another. However, where the saving of time is essential and the treatment is conducted on a larger scale, additional apparatus is necessary for the most economical and convenient treatment. If the timbers are of such size and form that it is practicable to change them from the hot to the cold bath, two tanks are necessary, one to contain the hot preservative—preferably equipped with steam coils—and the other to contain the cold preservative. For timbers too large to be transferred during treatment, the plant should consist of a treating tank, a supply tank, and a receiving tank. This equipment will make it possible to shorten the treatment by running out the hot liquid into the receiving tank and admitting the cold liquid simultaneously. The preservative can later be pumped back into the supply tank.

In most cases a tank built from three-sixteenths or one-fourth inch iron or steel will give the best results. Where steam can be supplied at a nominal cost, steam coils are preferable for heating.

THE ACTION OF ALCOHOL.*

A PHYSICIAN'S ESTIMATE.

BY A. R. CUSHING, M. D.

Alcohol affects so many functions of the body that it is impossible to take them all up, and I shall therefore limit my observations to its action on nutrition, on the brain, and on the circulation.

Its effects on nutrition are ascribed in part to its influencing the digestion and absorption of ordinary foods, and in part to its acting as a food itself. As to the first, its effects on the digestive processes, there is a very general belief, supported by apostolic authority, that alcohol improves digestion. A large number of painstaking investigations have been carried out without fully confirming the popular view, however. It is true that the digestive ferments have been found to be accelerated by very small quantities of pure alcohol, but the acceleration is very small in degree, and is not observed when the forms of alcohol in ordinary use are substituted for the pure spirit, nor when this itself is added in quantities corresponding to one to two glasses of whisky in man. As to the secretion of the digestive fluids under alcohol, it has been repeatedly found that alcohol after absorption induces a flow of gastric juice, but this is devoid of, or poor in ferments, and can scarcely promote the preparation of food for absorption. The movements of the stomach and intestine may perhaps be more active after alcohol has been swallowed in a concentrated form, and this may perhaps be the basis of the after-dinner liqueur. And some evidence is presented that alcohol accelerates absorption from the intestine, as is true of many other slightly irritant bodies.

The effects of alcohol on the digestion are therefore complex, for almost every factor in the process is altered in activity to a small extent when moderate quantities are taken. When one remembers, in addition, that the greatest influence of all on digestion is exercised by the taste and odor of the food, one is prepared for great divergences in the effects of alcohol in man. And this is the only inference that can be drawn from the results of clinical inquiry as to the value of alcohol as a stomachic in man. In some people the progress of digestion seems to be rather improved when wine is taken, but the improvement is small, while in others the reverse is true: the digestion is not accelerated, and may even be retarded by a couple of glasses of wine. The actual amount of food absorbed from the alimentary tract is scarcely changed definitely in either direction. But however variable the reaction of the individual stomach to small quantities of alcohol may be, there is only one response when larger quantities are taken, and that is disorganization of the whole process. And while the usefulness of alcohol in treating some digestive disorders may still be uncontroverted, Binz's dictum, that the healthy stomach needs no stomachic, and therefore no alcohol, must be the standpoint of the physician today.

But it may be argued that while the absolutely normal stomach is capable of digesting thoroughly and extracting the whole of the nutriment from the food offered to it, the artificial conditions under which most of us live necessitate measures which are unnecessary in a more healthful environment. Does not the jaded appetite demand exceptional measures, and may not wine be used to render food palatable and promote gastric secretion and digestion just as other condiments are employed? Shall he who seasons his food with wine be condemned, while he who substitutes mustard may be regarded as of upright life and conversation? The objection to the habitual use of alcoholic beverages "for the stomach's sake," in the scriptural phrase, does not arise from its effects on the digestion, but from the tendency toward the habit being formed and from its action on the brain.

In regard to the second phase of the question of alcohol on nutrition—its food value—the last few years have witnessed experiments performed with such meticulous care that there can be but one opinion in the minds of those who have studied the subject. Over 95 per cent of the alcohol ingested undergoes combustion in the tissues and is utilized by them as a source of energy. As regards its fate in the body, in fact, alcohol is strictly comparable to sugar, which is also an alcohol, though of a more complex nature than the substance under discussion. Like sugar, it is capable of supplying energy, which may be utilized in heat formation and mechanical work, and it may lead to the deposit of fat and to economy of the precious nitrogenous stores of the tissues. In fact, the more closely the metabolism under alcohol is examined, the more closely is it found to conform to that under an equivalent amount of carbohydrate, and it seems to me that the protagonists in the fight against alcohol can only harm their cause by still refusing to accept these results. For the recognition that alcohol resembles sugars and fats in its fate in the tissues by no means implies that it is a suitable food in disease or in health. The same is true of vinegar and even of morphine under certain conditions. Before an oxidizable substance can be contemplated as a substitute, therefore, it must be shown that, given in quantities which have any significance in calories, it has the greater possibilities for harm than normal foods. Can alcohol be taken without toxic effects on the tissues in general, quite apart from those on the brain and more specialized organs? On this point experiment has shown that when alcohol is substituted for other forms of food in persons unaccustomed to its use, it sometimes fails to act as an equivalent for some days, during which the deficiency has to be made up by the combustion of other available sources of energy. This has been supposed to indicate some specific toxic action of alcohol on the tissues; but there are grounds for believing that alcohol does not stand alone in this relation, but that the same phenomenon occurs when other unaccustomed but unimpeachable forms of food are suddenly substituted for those which the tissues have hitherto consumed. In this point, then, alcohol appears to behave in the same way as other nitrogen-free foods.

But another series of investigations has shown that alcohol leaves the tissues in an impaired state in regard to the measures which the organism normally takes to protect itself from injuries either by living organisms or by poisons. No experiments were needed to show that the abuse of alcohol lessens the resistance to invasion by pathogenic organisms; the records of pneumonia in our hospitals and of cholera in the East indicate this beyond the shadow of a doubt. But these fail to show beyond question that the habitual use of alcohol in small quantities, or its therapeutic use, has this effect; and the whole interest of the question at present lies in the dietetic use of alcohol as contrasted with the drunkard's abuse of it. A number of animal experiments on the subject have been performed by inoculations of pathogenic bacteria, or of their toxines, before or after the administration of alcohol, the mortality being compared with that of animals inoculated in the same way but not treated with alcohol. In these the results have almost uniformly been arrayed against the use of alcohol, but, unfortunately, the quantities of alcohol given scarcely correspond to the moderate use in man. Even Laitinen gave his rabbits about 1 c.c. per kg., which would correspond to about 4 to 5 oz. of whisky per day in a man. This quantity was insufficient to cause symptoms of intoxication in his animals, but the corresponding ratio could scarcely be considered within the limits of moderation in man.* Some recent work by Reid Hunt bears closely on this question, in that he finds that the continued administration of alcohol in small quantities renders mice and guinea-pigs much more susceptible to the effects of acetonitril, a poison which acts by the liberation of hydrocyanic acid in the tissues. Hunt considers his results due to alcohol deranging the metabolism in certain directions, notably tending toward an acceleration of some oxidative functions. And in this he sees a specific difference between alcohol and the carbohydrates, in that these have of course no such influence on the metabolic processes.

Hunt states that these changes arise from quantities of alcohol which correspond to that indulged in by "moderate drinkers," inasmuch as his animals showed

* Science Progress.

* Since this paper was written, a further series of experiments has been published by Laitinen, who finds that the prolonged administration to animals of quantities of alcohol corresponding to less than a glassful of whisky or two glasses of port wine per day in man, exercise a distinctly deleterious action on the blood, and reduce the resistance to infection.

no signs of intoxication. But a drop of alcohol per day for a mouse corresponds to an immoderate use in man; and in view of the importance of his evidence, it is extremely desirable that a more accurate determination of the amounts of alcohol given were available. For while I think there can be no question that alcohol in excess reduces the resistance of the general tissues to disease and may be deleterious to them in other ways, as Hunt's results indicate, it still remains undecided what is the lowest point in alcohol administration capable of inducing these effects.

On the whole, it must be conceded that small quantities of alcohol have not been proved to act deleteriously on the tissues in general, but, on the other hand, the threshold below which alcohol is innocuous has not been ascertained with accuracy, and probably lies nearer the limit of "moderate use" than is generally recognized.*

The effects of alcohol on the central nervous system differ very considerably in different individuals. In the lower animals they are marked by depression, the symptoms being exactly similar to those of chloral or of the other narcotics. Occasionally, a transient excitation occurs, but this is due to the irritant action in the stomach, and is absent when more dilute solutions are administered, or when the alcohol is ingested in small repeated doses. In human beings the same depressant effect is often seen, the first symptoms being more or less drowsiness and heaviness, which subsequently pass into slumber. But in the majority of cases the first effects elicited are feelings of good fellowship, well-being, and liveliness, with increased confidence in the mental and physical powers. The face is flushed, the eyes bright, and the pulse and respiration are accelerated. Larger quantities lead to uncertainty and inco-ordination of the movements, shown in difficult and stammering speech and staggering gait, and still larger amounts induce sleep and narcosis, which may pass into complete anæsthesia, and eventually prove fatal. The symptoms of intoxication, therefore, appear to differ in the lower animals and in man, for while in the former there are generally no signs of excitement, in the latter this feature may be very marked in the beginning. And even in man the effects of alcohol offer marked contrasts in different individuals, and in the same individual at different times. For the excitement is very much more marked when drinking is indulged in in company with others, while when alcohol is taken with less exhilarating environments or in solitude, the excitement stage is very much less marked or may be entirely absent.

The effects of alcohol on the brain have been explained in two different ways: the view which appears at first sight to be the more natural one, is that it first stimulates the cerebral cells to greater activity and then depresses them; that is, alcohol is believed to act on the cerebral gray matter in the same way as strychnine on the spinal cord. And this view, that alcohol is a central nervous stimulant, is still widely entertained both by the laity and by clinicians, while the majority of experimental observers rather lean to the view suggested by Schmiedeberg and supported by Bunge, Kraepelin, and many others, that the stimulation of the brain is only apparent. According to this theory, the excitement is caused, not by the augmented vitality of the nerve cells, but by a loss of the associations which ordinarily retard the expression of mental activity.† To adopt a mechanical simile, the brain may be compared to an engine fitted with powerful brakes. An acceleration of the motion may be due either to increased power of the engine (stimulation) or to the brakes being taken off, and it may be difficult for an onlooker to determine which is the true explanation. The apparent evidences of increased mental activity under alcohol, however, have proved for the most part illusory, when carefully investigated. The closer one approaches to the engine, the more evident it becomes that what appeared to be the result of increased power is really the effect of the removal of the brakes.

One argument against the stimulant action of alcohol is the narrow limits to which it is confined. All the recognized central nervous stimulants act on some particular part in small doses, but when larger amounts are ingested the stimulant action spreads over a wider area of the central nervous system, and gives rise to the symptoms characteristic of stimulation of that area. Caffeine, for example, first stimulates the mental functions apparently, but in larger quantities involves the motor sphere of the brain, and may finally stimulate the cells of the cord. Alcohol, on the other hand, appears to have only a depressant action on the nervous tissues, except in the human cerebrum. It is true that the exceptional development of the human brain might permit of a departure which is without analogy in other forms of poisoning.

But when the excitement stage is more closely investigated, it becomes apparent that all the cerebral functions are not facilitated by alcohol. It is common knowledge that under the influence of alcohol an individual may be more brilliant in conversation, more witty, more social, more generous in sentiment, but that he is not so careful in his statements, and has not that consideration for his own position or that of others which he ordinarily manifests.

The normal adult, in an environment which in a child would cause every symptom of exhilaration, maintains his self-control, partly because the position has lost its novelty, that is, he has associations which are wanting in the child, partly because the exuberance and gesticulations natural to the child are kept in check by his sense of the ridiculousness of these manifestations in later years. Remove the inhibiting associations, destroy the self-control of adult life, and the man becomes a child again in his sense of exhilaration and symptoms of excitement. The symptoms of intoxication which are ordinarily regarded as indicating stimulation of the brain do not necessarily involve this interpretation, but may be explained by a removal of those associations which ordinarily inhibit the external manifestations of emotion.

In a word, it is generally recognized that some of the highest functions of the brain are thrown out of action by alcohol administered in quantities which induce the phase of exhilaration. The further question is: What functions are actually increased in activity, and how far is this increase dependent upon the reduced activity of the processes which are depressed by alcohol?

Much valuable evidence as to the effect of alcohol on mental processes has been gathered in the psychological laboratories, especially by Kraepelin and his pupils, who have subjected the question to a large number of ingenious psychological tests. Thus it is found that typesetters do a smaller amount of work and make a much larger number of misprints when even a couple of glasses of beer are allowed, than when they perform their work without this drug. When a student was set to learn a number of meaningless syllables or a row of figures, he took a longer time to do so and made more errors in repeating them under alcohol than normally. Similarly, arithmetical calculations of all kinds were carried out much more slowly and with more errors, and the writing was slower and gave the impression of being that of an uneducated person who seldom had occasion to use his pen. Only in one respect was any increased aptitude shown—namely, in the transformation of an idea into movement. And many ergographic experiments appear to show that small quantities of alcohol have the effect of temporarily increasing the capacity of doing muscular work, especially when the subject is fatigued. This augmentation is only transient, and the total work done in the course of the day is considerably reduced by alcohol, as has long been demonstrated in the case of forced marching. These might suggest the view that the motor cells are first put in a state of greater activity by the direct action of alcohol on them, but another explanation is equally applicable, namely, that an ordinary movement is hampered by a series of associations, and that when these are destroyed by alcohol the movement may be carried out more quickly. At the same time, the associations in ordinary life not only retard the movement but also restrict and direct it, and the result of their absence under alcohol is shown by the awkwardness and inaccuracy with which the movement is executed.

It is noteworthy that in these experiments those mental processes which were ordinarily performed readily were less retarded than others in which the subject was less practised and which required more effort. That is, the powers most recently acquired and most readily lost are those on which alcohol first acts, while those operations which have become habitual are less impaired. This is in complete accord with what is observed in the earlier stage of intoxication or exhilaration. The most recent acquisitions in adult life are the power of self-control and the feeling of self-respect, which are manifested in regard for the conventions of life, and in the prudence which leads one to avoid many procedures which in earlier life might have been indulged in without reproach. And under alcohol these are the first mental processes to be disordered. *In vino veritas*, in intoxication the natural man is exposed, stripped of the trammels of convention, and robbed of the fruits of experience and education.

Many other results of these experiments are of interest, but cannot be entered on here. One feature which bears a familiar aspect, and which was brought out clearly in many instances, is the confidence of the subject of the experiment that his efforts were unusually successful under alcohol, while the impartial record showed results far below the level attained during abstinence.

In all of these experiments the amount of alcohol taken was small, in no case sufficient to induce any of the more evident symptoms of intoxication. It is therefore of great interest to find in the later publications of Kraepelin's laboratory, that distinct impairment of the physical powers was found to persist for a much longer time than would be anticipated; the average efficiency was not regained until from twelve to twenty-four hours had elapsed after the alcohol had been taken. If this proves to be true in the case of persons who have acquired a tolerance for alcohol, even moderate drinkers are never completely normal, their mental powers never recovering entirely from one dose of alcohol before the next is taken.

The results of these investigations on the mental state under alcohol, as well as many others which I have not discussed, appear to place the theory that alcohol acts as a narcotic upon a firm basis. I do not hold that it is absolutely determined, but some more satisfactory evidence must be brought forward in favor of the stimulation theory before it can be considered as a rival to that of Schmiedeberg. And this evidence must define what functions are stimulated, and must not be a repetition of the old statement, that the tone and vigor of the nervous system are maintained by alcohol.

But if the chief or even the whole action of alcohol on the brain were proved to be a narcotic one, this would not preclude its use in therapeutics. For here the chief object in the use of alcohol is not to induce but to repress cerebral activity, and the alleged stimulant action arises from a confusion of ideas; a similar confusion is met with in regard to opium, which was also regarded as possessing stimulant properties which recommended its use in acute diseases accompanied by cerebral symptoms. The result of both drugs is in reality cerebral depression, which manifests itself in a condition of euphoria. The disease often loses its strongest ally when the anxiety and worry of the patient are allayed by alcohol. He feels less concern and more resignation, not because his soul rises triumphant through the stimulant action of alcohol, but through his brain being less capable of dwelling on his disabilities, because it is partially narcotized.

Alcohol, therefore, is not to be regarded as exercising any unique action on the brain in disease, but resembles the other narcotics, and as a remedy must be compared with these. And on the whole it emerges from the ordeal as not inferior in many points. Thus, none of them disturb the digestive tract less, and most of them exercise a more depressant effect on the centers in the medulla oblongata; for I think it may be taken as proved that in ordinary therapeutic doses alcohol does not depress the respiration, but, on the whole, tends to augment it even when no excitement is induced. Where a slight narcosis, sufficient to allay anxiety and induce rest, is required, then alcohol deserves consideration, but it is less certain in effecting a more marked depression, for which it cannot be compared with the modern narcotics.

The effects of alcohol on the circulation have long been a matter of dispute, many clinicians holding that in failure of the heart and vasomotor center alcohol is of service, while others have found that it may be dispensed with. The experimental results have recently been discussed by Dixon, who suggests that alcohol may support the heart by acting as a foodstuff, and may thus increase the blood-pressure and improve the circulation. It is true that another observer has not been able to satisfy himself that alcohol acts as a food for the heart, but Dixon's results unquestionably re-open the question of the usefulness of alcohol in the treatment of acute circulatory failure, in regard to which a skeptical attitude has more recently been adopted by many of the leaders of the medical profession. And in addition to any direct action on the circulation, alcohol may prove of value in circulatory failure through its narcotic action in the same way as opium, over which it has the advantage of not inducing any embarrassment of the respiration.

To sum up, alcohol may be of some value in therapeutics as an adjuvant to foods, in order to render them more attractive, and thus improve their digestion, and also possesses some food value itself. It acts as a cerebral depressant, and may be useful in certain diseases in this way, and it may perhaps aid the failing heart. A small dose exercises no definite demonstrable poisonous effects on the tissues. If alcohol were a new synthetic remedy fresh from Germany, it might probably be hailed as a useful addition to therapeutics, but its popularity as a remedy would be short-lived. Not because of its failure to substantiate its claims as a narcotic, nor because it had proved fatal in the hands of physicians, but because an alcohol habit had been developed in some of the population. If one considers the few sporadic cases in which sulphonal or cocaine, for example, has given rise to a habit, and the discredit into which it has fallen in consequence, and compares the cases of disaster from

* In view of the statement made by Prof. Reid Hunt during the discussion of this paper and the later publication by Laitinen just mentioned, I feel that this paragraph requires modification. The very moderate use of alcohol (by which I mean the habitual daily consumption of one or two glasses of port, one or two pints of beer, or a glass of whisky) appears from the researches of these authorities to induce definite deterioration of the tissues, and to render them more susceptible to attack by disease.

† When it was propounded the rôle of inhibition in the central nervous system was not appreciated, or at any rate had not been experimentally demonstrated. The recent work of Sherrington has, however, shown that every movement involves motor and inhibitory impulses, and seems to give a firm basis for the depression theory of alcohol.

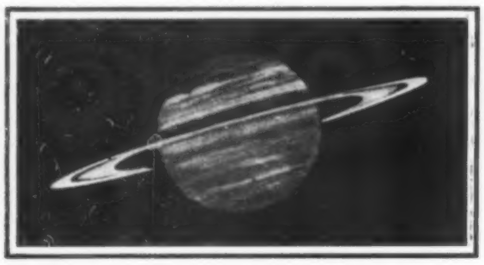
alcohol met with every day, one cannot but feel that the drugs have been weighed in very unequal scales, and that any one who discards cocaine—undoubtedly a drug of great value—must, to be consistent, condemn the use of alcohol. This seems to me to be the ground on which we must take our stand. Alcohol is a drug which may be useful in therapeutics, although I think it can hardly be considered indispensable, but which has so often given rise to habit that its use must be curtailed to the utmost limit. In some conditions, as in old age and debility, it may be justifiable to neglect its drawbacks, exactly as in some forms of malignant disease the patient may be allowed to contract the morphine habit; but let us at any rate advise it here with eyes open to the risks run, and with the recognition that we are prescribing a drug and not merely a placebo. It may be argued that alcoholism has almost never arisen from the therapeutic use of the drug, and it is true that the responsibility for alcoholism does not rest upon the medical profession in the same way as that for morphinism or cocainism. But the public are justified in regarding alcohol not as a drug, but as an article of diet, as long as the physicians order it in the casual way which is familiar to all of us. How often is alcohol not ordered but allowed by the medical man at the suggestion of the patient, and how many physicians allow it, not believing that it will do any good, but assuming that it will do no harm, and even not hesitating to make the statement!

The laity can hardly be blamed for ignoring the evidences of danger presented daily in their streets, if their scientific mentors themselves adopt this ambiguous position, and the medical profession cannot complain if they are accused of indifference toward the greatest evil of their country and their age.

SATURN AND ITS RINGS.

SEEN FROM VARIOUS POINTS OF VIEW.

The accompanying drawings of Saturn and its rings, as viewed from his nearest satellite and from the earth, were made by the Abbé Moreux, of the observatory of Bourges. The same astronomer gives, in Cosmos, the following popular explanation of the repeated disappearances and reappearances of the rings



APPEARANCE OF SATURN AND ITS RINGS ON JULY 16, 1906.

that have been observed during the past year and in former years.

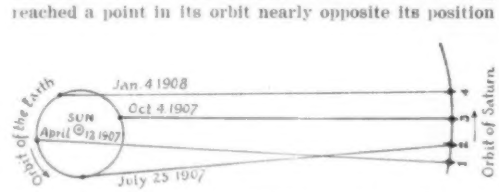
In the year 1610 Galileo first observed Saturn with a telescope magnifying more than 30 diameters, and was astounded on seeing the planet apparently flanked by two small companions, almost in contact with it. With lower power the three supposed planets were merged into one of oval form. Two years later the mystery deepened, for the planet appeared as a perfectly circular disk, without companions. Galileo doubted the correctness of his earlier observations and abandoned the study of Saturn.

A person knowing nothing of Saturn who had observed the planet during the past few years with Galileo's primitive instruments would find himself in a similar state of perplexity.

The rings of Saturn were first recognized as rings by Huygens, forty years after Galileo's observations. They are parallel to the planet's equator, but inclined about 27 deg. to the plane of his orbit and 28 deg. to the ecliptic, their nodes being at longitude 168 deg. in Aquarius, and at longitude 348 deg. in Leo. As the plane of the rings remains sensibly parallel to itself for a very long time, it follows that for fifteen years (half a revolution of Saturn) we see their northern face and during the remaining half revolution we see their southern face. At the epochs of transition, when the earth passes through the plane of the rings, their edge is presented to us. This occurred last year and in 1612. As the thickness of the rings is less than 100 miles, their edge was quite invisible in Galileo's feeble telescope.

The disappearance of the rings recurs at intervals of about fifteen years. In 1891 the phenomenon could not be well observed because of the angular proximity of Saturn to the sun. Afterward the rings appeared as a narrow ellipse which gradually expanded, attaining its maximum breadth in 1899, and then gradually narrowed until last year, when the first disappearance of the rings occurred on April 12.

The expression "first disappearance" requires some explanation. The accompanying diagram shows the positions of Saturn and the earth at four critical epochs during the past year, April 12, July 25, and October 4, 1907, and January 4, 1908. On April 12 the earth was in the plane of the rings and consequently the rings could not be seen except with very powerful telescopes. On July 25, on the other hand, the plane of the rings passed through the sun so that neither face was illuminated and hence the rings again became invisible. On October 4, the earth, having reached a point in its orbit nearly opposite its position



RELATIVE POSITIONS OF SATURN, THE EARTH, AND THE SUN IN 1907–1908.

in April, again passed through the plane of the rings which, therefore, disappeared a third time. Finally, on January 4, 1908, the earth passed through the plane of the rings for the third time, causing the fourth and last disappearance of the rings. They reappeared soon afterward and they will grow broader and more conspicuous until the end of the year 1914, and thereafter will decrease in width until they vanish again about fifteen years from now.

Saturn has been carefully observed by many astronomers during the past year because this period of disappearances affords a favorable opportunity for the study of the structure of the rings, and the observation of certain singular phenomena that are peculiar to this period. Our Paris correspondent supplies a description of these phenomena as they were observed by Amann, at Aosta. From the diagram, it is evident

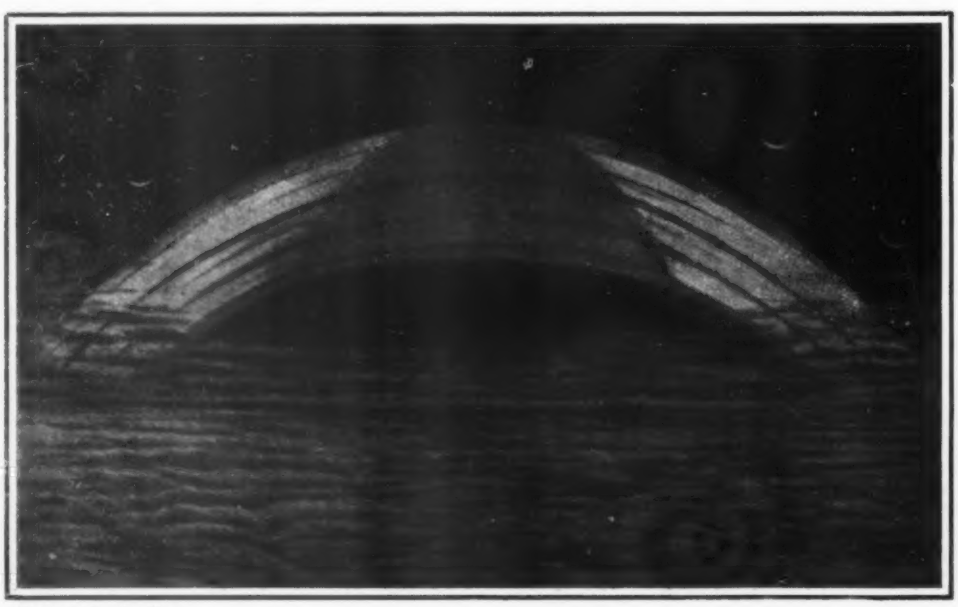
APPEARANCE OF SATURN AND ITS RINGS IN DECEMBER, 1874, SHOWING THE INNERMOST GAUZE OR "CRAPE" RING AND THE SHADOW OF THE PLANET ON THE RINGS.

that from April 12, when the plane of the rings first passed through the earth, to July 25, when it passed through the sun, it must have passed between the earth and sun, so that the unilluminated face of the rings was presented to our view. The same conditions prevailed from October 4, 1907, to January 4, 1908. It might naturally be supposed, therefore, that the rings would be totally invisible during the whole of each of these periods. This, however, was not the case. In the second period, Amann saw the rings many times with a 7-inch and even with a 4-inch telescope. On November 11, a week after the vanishing of the rings, a faint glow appeared on the east side of the planet. This was followed by a bright line extending on both sides to the width of the outer ring. After November 15 the ring was clearly visible as an ellipse, which attained its greatest breadth about November 23, and then became again reduced to a line on December 18.

From December 18 to January 2, 1908, the rings were represented by a faint band traversed longitudinally by a black line, which seemed like a prolongation of the shadow of the ring in the planet's disk. The part of the band which lay north of the dark line vanished on January 4, but the southern part remained visible until January 6. The ring varied considerably in brightness during the period of observation, being very faint in October and the beginning of January and of maximum brightness about December 1. It resembled moonlight in color throughout the entire period, and was never as bright as the satellites Rhea and Dione.

Amann continued his observations after the earth had passed through the plane of the rings and their illuminated face was again presented. A faint glow appeared on January 7, but (partly because of the weather) the rings were not clearly visible until January 10, when they appeared as a faint narrow and pointed band, each half of which showed two bright points, placed symmetrically with respect to the planet. During the next three days the appearance was that of a row of points of unequal brilliancy, of which some were larger and brighter than the satellite Titan and some were as bright as Saturn himself. All these points exhibited a rapid scintillation which could not have been of atmospheric origin as the air was very still. The number of bright points diminished from day to day. The last of them vanished on January 17. From the 10th to the 13th, the ring appeared copper colored. After January 13 it became brighter and assumed the yellow hue of Saturn's disk.

Bright points or "knots" in the rings were seen with the great American telescopes during each of the periods when the sun and the earth were on opposite sides of the plane of the rings and have been noted by various observers during former passages of the plane of the rings through the earth's orbit. (Amann, however, appears to have seen them after, and only after, the sun and the earth had come again to the same side



Illustrated London News.

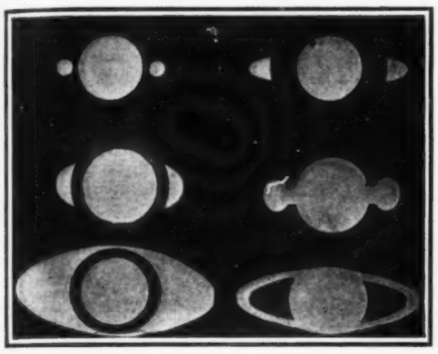
SATURN'S EVENING SKY AS SEEN FROM A POINT ABOVE THE CLOUDS.

The shadow of the planet falls on the rings, which span the sky like a rainbow. Three crescent moons are also shown.

SATURN AND ITS RINGS.

of the plane of the rings.) These and other phenomena have been discussed in recent issues of the SCIENTIFIC AMERICAN by Dr. Mitchell on November 23, 1907, and April 4, 1908, and by Prof. Lowell on December 14, 1907.

It had already been demonstrated, mathematically by Maxwell and spectroscopically by Keeler, that the rings are not continuous, but are composed of separate particles, like a swarm of meteors. Hence Prof. Barnard explains the visibility of the rings when the sun and the earth are on opposite sides of their plane



THE FIRST DRAWINGS OF SATURN, MADE IN THE SEVENTEENTH CENTURY, BY GASSENDI AND HUYGENS.

Some of these early drawings explain Galileo's supposed discovery of two "companions," or fixed satellites, in 1610.

by the filtration of sunlight through this cloud of cosmical dust and he regards the knots as the result of irradiation of parts of the cloud which are denser but not necessarily thicker than the rest under the illumination of sunlight which comes to them through adjacent portions of less density. According to Prof. Lowell, however, the rings are not flat and of uniform thinness, but rather resemble a concentric series of tores, or anchor rings, and the knots represent their thickest portions.

THE EARTH AS A CLOCK: ITS ROTATION.

By PROF. POYNTING.

THE sun, moon, and stars appear to go round the earth once in about every 24 hours. Formerly it was believed that they did go round thus, and that the earth was at rest. Now we know that the appearance is due to the earth spinning round past the heavenly bodies.

If we seek to describe mere relative position—how the sky appears from the earth—it is quite as correct to say that the sky is turning round the earth, as to say that the earth is turning round under the sky; both ways of describing the change lead to the same result. But when we consider the forces acting on bodies to change or guide their motion, then one mode of description gives us a consistent and conceivable scheme, while the other mode is inconceivable.

When a body moves in a circle a force is needed to act on it, pulling toward the center, proportional to the square of the rate of spin and proportional to the distance. Every pound of mass in the moon would need a pull equal to about the weight of 3 ounces to keep it in a circle which it traveled round once in 24 hours. Every pound in the sun would need a pull about three-quarters of a hundredweight. The nearest fixed star would need a pull on every pound, about 9,000 tons. We cannot imagine that the earth could exert such forces, and so are obliged to think that the earth is revolving and not the sky.

There is other evidence of its revolution to be found in the bulging at the equator. Again, when the air flows from all sides into a region of low pressure it always circles round the region counter clockwise in the northern hemisphere, forming a "cyclone" or immense whirlwind. If air is coming from the north it is coming from parts of the earth moving less rapidly toward the east than those which it reaches, it lags behind and so goes to the west of the center. If air is coming from the south, it is coming from parts of the earth moving more rapidly toward the east than those which it reaches; it keeps some of this motion and so goes to the east of the center. Thus the spin is started. It is easy to see that a cyclone in the southern hemisphere whirls round in the opposite direction.

Foucault's pendulum gives another proof. A very long pendulum is set swinging. The rotation of the earth makes the floor turn round under it, and the pendulum appears to change its plane of swing.

The earth is a clock. The line to the sun is the finger and the sky is the face. But the sun is not a regular timekeeper. Our twenty-four-hour day is only the average between successive noons or times when the sun is due south. If compared with a good clock the sun is in parts of the year too soon and in other parts too late, sometimes as much as a quarter of an hour. This cannot be due to change in the earth's rate of spin. For to change a spin there must be either a force acting at one side of the center of gravity, or a change of shape. The forces on the earth exerted by the sun and moon act almost exactly through the center of gravity, and so affect the rate of spin hardly at all. The earth does not change its shape sufficiently to account for the variation in the solar day.

The variation in the solar day is due partly to the inclination of the earth's axis to the plane in which it moves round the sun, partly due to the variation of motion of the earth's motion round the sun at different times of the year.

The fixed stars keep good time, getting round in about 4 minutes less than 24 hours. By them clocks are rated. Their day is the true time of one revolution of the earth.

The forces exerted by the sun and moon are not quite through the center of the earth. Consider the case of the moon. It raises tides on the earth, one tide on each side. These tides are not immediately under and opposite the moon, but are carried forward, as it were. The moon pulls the nearer one rather more than the further one, and the small extra pull acts as a brake, gradually lessening the earth's spin and lengthening the day.

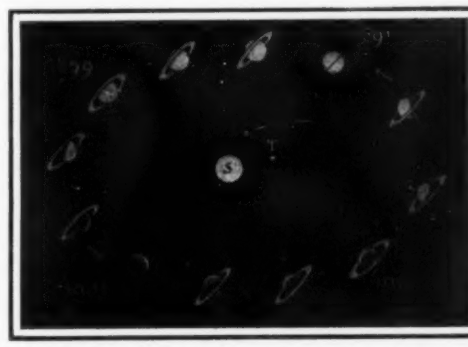
In the thousands of years of which we have observations of eclipses the effect has been hardly noticeable, but it must exist.

A similar action has been exerted by the earth on the moon, forming tides on it, and stopping its spinning until it always presents the same face to the earth.

The tides react on the moon, and it can be shown that the effect is gradually to drive the moon further and further away. The joint effect is to lengthen the earth's day and the moon's month.

Looking back to the past, the day and month must have both been shorter than now and the moon nearer. Calculation shows that when the day was somewhere about four hours the moon was near the earth, and the month was four hours too. Probably the moon still earlier was a part of the earth which broke away and became a satellite.

Looking forward the day and the month will both grow longer, till each is about fifty-five of our days.



PHASES OF SATURN'S RINGS, AS SEEN FROM THE EARTH AT VARIOUS EPOCHS.

S. The sun. T. The earth.

Then the moon and the earth will present the same faces toward each other, and the lunar tides will have ceased.

But the process is immeasurably slow. The birth of the moon may have occurred hundreds of millions of years ago, and it may go on lengthening its period of revolution for hundreds of millions more.

Most works on elementary astronomy give some account of time.

"Time and Tide," by Sir Robert Ball, gives an account of the action of the moon in lengthening the day.—Syllabus of a lecture delivered at the University of Birmingham.

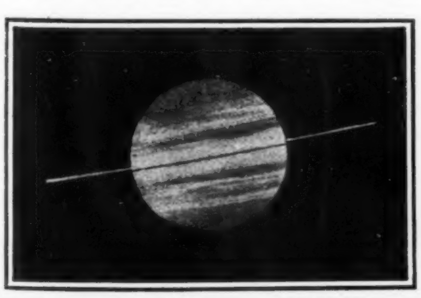
PARIS TO OWN THE EIFFEL TOWER.

NEXT January the Eiffel Tower will become the property of Paris, in accordance with an agreement entered into in 1889 with its contractor and architect, M. Eiffel, that the big steel structure should belong to the city after twenty years. M. Eiffel deposited a bond of $200,000 to bind the agreement, and this money will now go to his heirs.

What Paris will do with the tower is a problem. Admission fees and the sublet contracts to the restaurant hardly pay 1 per cent of the capital invested. It took eleven years for M. Eiffel to recoup his expenses, notwithstanding the great exposition of 1889, which brought thousands of visitors to the tower.

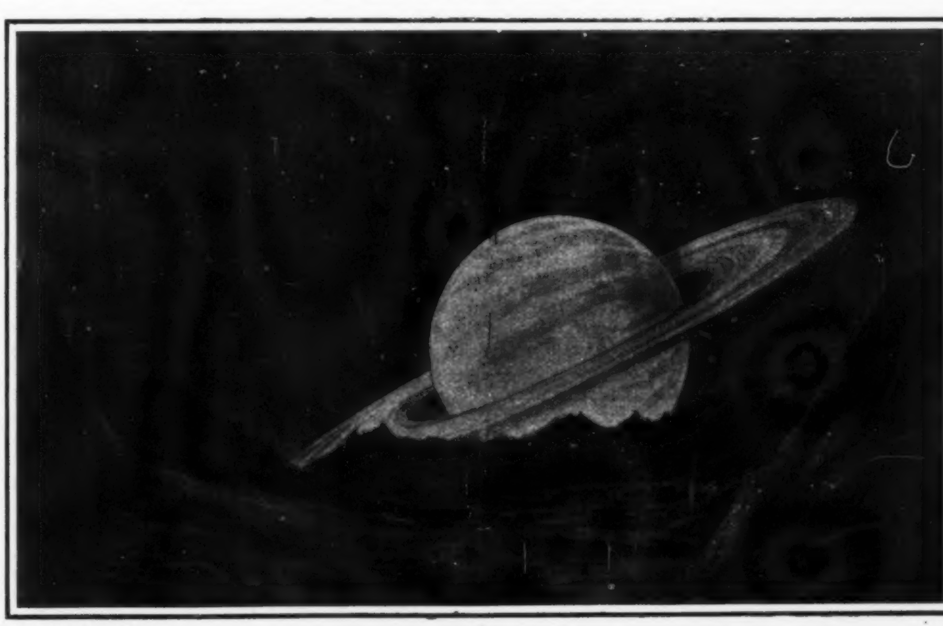
The entrance fee now is only twenty cents, hardly enough to cover the cost of running the huge elevators, and the city will have to devise some scheme to make the tower pay.

There is now a subsidy paid yearly by the French government on account of the wireless connections established there, but this is so small that it is not worth considering. It is generally believed that the tower will prove a white elephant in the hands of the municipality.



THE RINGS SEEN AS A BRIGHT, AND THEIR SHADOW AS A DARK STRAIGHT LINE, ON JULY 30, 1907.

DISAPPEARANCE OF THE RINGS IN THE BEGINNING OF JANUARY, 1908. THE BLACK LINE REPRESENTS THE SHADOW OF THE RINGS ON THE PLANET.



Illustrated London News.

SATURN RISING, AS SEEN FROM HIS NEAREST MOON.

The planet and its rings fill a large part of the sky.

SATURN AND ITS RINGS.

THE INSTINCT OF FEIGNING DEATH.*

A PROTECTIVE DEVICE EMPLOYED BY SOME ANIMALS.

BY PROF. S. J. HOLMES, UNIVERSITY OF WISCONSIN.

The so-called instinct of feigning death is one which is very widely distributed in the animal kingdom. It crops out sporadically, as it were, in forms which are but very distantly related, and hence it must have been independently evolved a great many times. The expression feigning death is a misleading one to the extent that it is apt to give rise to the idea that the animal consciously adopts this device with the intent to deceive. While it is probable, however, that among the higher animals which sometimes feign death there may be an attempt to mislead their enemies, it is quite certain that among the insects, spiders, and other low forms there is no such aim in the creature's mind if we grant (what some naturalists are disposed to deny) that these animals have minds. The veteran French naturalist, Fabre, who has devoted the leisure periods of a long life to the enthusiastic study of the ways of insects, performed several experiments on beetles in order to ascertain if the duration of their feint was in any way affected by his own presence or movements. Most of Fabre's observations were made on a large scarab beetle. When handled the beetle would throw itself into an immobile state with its head bent down and its legs drawn in close to the body. It would remain in this attitude perfectly quiet for several minutes—sometimes for over an hour. Its awakening would be first manifested by a slight trembling of the feet and a slow oscillation of the antennæ and palps; then its legs would move about more vigorously, and finally the insect would arise and scamper off. Seized again, it would repeat the performance several times in succession, the duration of the feint often increasing with successive trials. Finally, as if wearied, or convinced that the ruse were vain, the beetle would refuse to feign longer.

Were the feints attempts to deceive its captor by simulating death? Fabre placed the insect on its back, went to a distant part of the room and remained perfectly quiet. The beetle still lay as usual. He then went out of the room, carefully looking in at intervals to watch the course of events. Still the same immobility. In other cases he covered the insect so that it could not see out and then quietly went away. This was also found to make no difference. In fact, whether the insects were surrounded by sounds and sights of moving objects or entirely excluded from these influences made no difference in the average length of time they would remain in a motionless condition. Similar experiments have been made on other insects by different observers, who have all arrived at the conclusion that conscious deception plays no part in the process.

The attitudes assumed by insects and other forms when feigning death are usually quite different from those of dead specimens. This general fact was pointed out by Darwin, who says: "I carefully noted the simulated positions of seventeen different kinds of insects belonging to distinct genera, both poor and first-rate shammers; afterward I procured naturally dead specimens of some of these insects, others I killed with camphor by an easy slow death; the result was that in no instance was the attitude exactly the same, and in several instances the attitudes of the feigners and of the really dead were as unlike as they possibly could be."

The attitudes of animals in the death feint are frequently very characteristic. Many beetles as well as other forms feign with the legs drawn up to the body and the antennæ closely appressed, so that the whole insect assumes as compact a form as possible. The woodlouse, *Armadillo*, rolls itself up into a ball with its legs drawn into the center, a habit which has doubtless caused the name pill-bug to be given to this crustacean. A beetle, *Geotrupes*, according to Kirby and Spence, "when touched or in fear sets out its legs as stiff as if they were made of iron wire—which is their posture when dead—and remaining motionless thus deceives the rooks which prey upon them. A different attitude is assumed by one of the tree-chafers probably with the same end in view. It sometimes elevates its posterior legs into the air, so as to form a straight vertical line, at right angles with the upper surface of its body."

Spiders usually feign by folding up their legs, dropping down and remaining motionless. The caterpillars of some of the geometrid moths have the curious habit of attaching themselves to a branch by their posterior legs and holding the body straight and stiff at an angle to the stem, thus forming a remarkably close resemblance to a short twig. Frequently the deceptiveness is increased by a marked similarity in color to that of the branch to which they are attached.

While in most cases a species has a particular attitude which it maintains when simulating death, there are some forms which feign in whatever posture they may be in when disturbed. A good example of this is afforded by the water-scorpion, *Ranatra*. This insect has the two hinder pairs of legs, which are employed in walking and swimming, very long and slender; the first pair are fitted for grasping the small aquatic animals on which it feeds and are carried straight out in front of the body. It is only necessary to pick one of these insects out of the water to throw it into a stiff, immobile condition which usually lasts several minutes and sometimes for over an hour. The legs may be closely pressed to the body so that the creature resembles a stick, or they may stand out at right angles to it, or be bent in any position, some in one way and some in another, depending upon how they happen to lie when the feint began. And no matter how awkward the position, it is rigidly maintained until the feint wears off. I have found that young *Ranatras*, the first day they emerged from the egg and while their appendages were still soft and easily bent, showed the same death-feigning instinct as the adults, although they did not persist in it for so long a time. It is a curious fact that the mature insect can not by any sort of manipulation be caused to feign death while under water; but as soon as it is in the air it can be caused to feign repeatedly; sometimes a slight touch is all that is necessary to throw it into a rigid state of an hour's duration.

Death feigning does not seem to occur among the lower invertebrate animals such as the Protozoa, Cœlenterates, mollusks, and worms, although some of them may exhibit reactions which are prophetic of this instinct. Among crustaceans the instinct in its fully developed form is quite uncommon. Some years ago I described the death-feigning of certain species of terrestrial amphipod crustaceans which are frequently found on sandy beaches near the seashore. On account of their peculiar hopping movements these crustaceans are commonly known as sand-hoppers or sand-fleas, although they have of course no relation to the ordinary fleas of human experience. One of the largest species of sand-hopper, *Talorchestia*, is common along our Atlantic coast, where it lives during the day in burrows made in the sand, coming out only at night to feed upon the seaweed and other material washed ashore by the waves. When the talorchestias are dug out of their burrows, they usually lie curled up with their long antennæ bent under the body and their legs drawn up so as to assume a compact form. They will lie in this way for several minutes, when they may be seen slowly to relax; the legs then move about, and soon the creature hops away by a sudden extension of its abdomen. When caught in the hand they will feign death again, and repeat the performance many times in succession. Other species of sand-hoppers exhibit the same instinct, though less perfectly, and there are traces of it in many of the reactions of their aquatic relatives.

The various species of wood lice exhibit the instinct of feigning death in various degrees. Some species are able to roll up into an almost perfect ball and will remain in that state for a considerable time. Other species curl up, but make only a very imperfect approximation to a sphere, and maintain this attitude for but a short period. Some myriapods when disturbed curl up in much the same way. Among spiders death-feigning is not uncommon, especially among the orb weavers.

It is among the insects that the death-feigning instinct reaches its fullest development, occurring to a greater or less extent in most of the orders. It is especially common in beetles and not unusual among the bugs, but it is quite rare in the highest orders such as the Diptera or flies, and the Hymenoptera, or the ants, bees and their allies. It occurs in a few cases among butterflies and moths, both in the imago as well as the larval state. The instinct is exhibited in different species in all stages of development from a momentary feint to a condition of intense rigor lasting for over an hour. Some insects may be severely mutilated, or, according to De Geer, even roasted over a fire before they will cease feigning.

Among the vertebrate animals death-feigning has been observed only rarely in the fishes. In the Amphibia it is not exhibited in the striking way it occurs in insects and spiders, although frogs and toads may be thrown by the proper manipulation into an immobile condition more or less resembling it. A phenomenon apparently related to the death feigning of insects has long been known in certain reptiles. Darwin in his "Journal of Researches" describes a South American lizard which when frightened "attempts to avoid discovery by feigning death with outstretched legs, depressed body, and closed eyes; if further molested it buries itself with great quickness in the loose sand." The Egyptian snake charmers by a slight pressure in the neck region are able to make the asp suddenly motionless so that it remains entirely passive in the hands of the operator. And similar phenomena have been found in other species.

In birds the instinct crops out only here and there. A few summers ago when on the island of Penikese I was somewhat surprised to find the instinct well developed in the young terns which were hatched out in abundance on the hillsides. For a short time after being hatched the little downy fellows betray no fear of man and will cuddle under one's hand in perfect confidence. When the birds become larger and acquire their second coat of feathers the instinct of fear takes possession of them and they run and hide in the grass when you approach. Here they lie perfectly quiet; you may pull them about, stretch out their legs, necks, or wings, and place them in the most awkward positions, and they will remain as limp and motionless as if really dead. They will even suffer their wing or tail feathers to be plucked out one by one without a wince. But all of a sudden the bird becomes a very different creature. It screams, pecks, and struggles to escape. I have made several attempts to make a bird feign death a second time, but never met with success. According to Couch the land rail and skylark feign death, and Wrangle states that the wild geese of Siberia have the same habit during their molting season, when they are unable to fly. Hudson states in his most interesting "Naturalist on the La Plata" that the common partridge of the pampas, when captured, "after a few violent struggles to escape drops its head, gasps two or three times, and to all appearances dies. If, when you have seen this, you release your hold, the eyes open instantly, and with startling suddenness and noise of wings, it is up and away beyond your reach forever."

In mammals the instinct is so well shown in one of the lower members of the group, the opossum, that the expression "playing possum" is familiar to every one. Foxes when trapped or hard pressed often drop down limp and apparently lifeless, and will even endure a good deal of maltreatment without making any response. Hudson records that he was "once riding with a gaucho when we saw, on the open level ground before us, a fox not yet fully grown standing still and watching our approach. All at once it dropped, and when we came up to the spot it was lying stretched out, with eyes closed, and apparently dead. Before passing on my companion, who said it was not the first time he had seen such a thing, lashed it vigorously with his whip for some moments, but without producing the slightest effect." Dogs are frequently deceived by this ruse of the fox and doubtless foxes have many times owed their lives to its aid. It has been noticed that if one withdraws from a fox when it is feigning it may be seen to slowly open its eyes, then raise its head and carefully look around to see if its foes are at a safe distance, and finally scamper off.

While in insects the instinct of feigning death is probably a simple reflex reaction to outer stimuli, it is doubtless associated in birds and especially mammals with a tolerably acute consciousness of the situation. It involves a more or less deliberate intention to profit by the deception, yet at the same time it is probably not a result of conscious reflection. The instinct is there, or else such a course of action would not occur to the animal's mind. Were it otherwise it would be difficult to understand why the ruse is adopted only by certain species while many others, equally intelligent and for whom it would be an equally advantageous stratagem, never manifest it. There can be little doubt that a fox which slowly opens its eye and warily looks around is acting with an intelligent appreciation of his predicament, but it is not to be inferred that he could have reasoned out his course of action did not an innate proclivity in that direction form a part of his instinctive make-up.

The physiological condition in what is called death-feigning is quite different in different forms. In most

* From Popular Science Monthly.

of the lower animals it is characterized by a tetanic contraction of the muscles. The attitudes assumed by many forms, such as rolling into a ball, keeping the legs and other appendages drawn close to the body, or in some cases holding them straight and rigid, are such as can be maintained only at the cost of considerable muscular effort. If a *Ranatra* is picked up by one of its slender legs it may be held out horizontally for a considerable time without causing the leg to bend. It is as if a man were seized below the knee and held out straight, face upward, without causing the knee to bend; only the legs of a *Ranatra* are several times more slender than those of the most attenuated of the human species, and the muscular tension which the insect maintains must therefore be intense.

The death feint of insects and other low forms is not entirely dependent on the brain. It is due rather to a general physiological state of the animal. I have found that the posterior part of the body of a *Ranatra* can still be induced to feign death, though less perfectly, when entirely removed from the head and prothorax. When it would come out of the feint a few light strokes would cause it to feign again. It has been found that spiders also may feign after entire destruction of the brain.

The instinct of feigning death is doubtless closely connected with much of what has been called hypnotism in the lower animals. Crayfishes, frogs, lizards, certain snakes, and many birds and mammals may by a very simple process be thrown into an inactive condition from which they are not readily aroused by external stimuli. In ordinary death feigning the animal falls into its immobile state upon slight provocation; a touch, or even a jar is sometimes all that is necessary. In the so-called cases of hypnosis more or less manipulation is necessary. The exciting cause in both cases is generally some form of contact stimulus.

In the hypnotism of animals, as Verworn and others have shown, there is diminished reflex irritability, and usually tonic contraction of many at least of the muscles. Similar phenomena are observed in the death feigning of many forms, some of the insects showing a lack of responsiveness that is truly remarkable. In a water-scorpion that is feigning death the legs may be cut off one by one, or the body cut in two without eliciting the least reaction from the unfortunate victim. We can only speculate at present on the condition of the nervous system which makes such a result possible.

THE WORK OF HUGO DE VRIES.*

A SIMPLE EXPLANATION OF HIS MUTATION THEORY.

BY PROF. HERBERT MAULE RICHARDS.

WHAT De Vries has really done is to bring within the range of experimental proof certain questions which heretofore have been regarded as matters of observation and speculation alone. From this point, which might be said to have had its origin in the acuteness of observation of the taxonomist and morphologist, the physiological trend has ever increased until the last word in this discussion may perhaps be for the physiologist alone. The great question involved in the Mutation Theory is the old, old problem of the origin of species, a very considerable advance in which has been made by De Vries and those who were stimulated by his work. It is quite wrong to suppose that he has controverted the general results of Darwin's work; he has supplemented it, brought it within the range of more conclusive proof.

As the Linnæan or collective species may be regarded to-day they are usually separable into several more or less distinct strains which show no intergrading forms, and the diagnosis of any one species is, so to say, the average impression of them. To these distinct strains De Vries has given the name elementary species, and according to his interpretation they are the really discrete, finally segregable units, between which no intermediate types exist and concerning the origin of which we are really concerned. It matters not whether it was through ignorance or simply from convenience that the earlier taxonomists grouped many of these forms into a single species; we must conclude, that in general species, as recognized by the books, are quite artificial. It matters not, also, what we call these finally not further resolvable forms. Therefore let us accept De Vries's terminology and use the term elementary species; the real point of the inquiry is, how did these forms arise? It is upon this that De Vries's work has thrown a great light. He has shown that they may arise suddenly and without previous preparation from pre-existing forms, in which case the elementary species may be termed mutants, and the theory which has to do with the investigation of their origin the Mutation Theory.

The next task then is to examine more closely the methods which De Vries employed, the evidence which he has to support his views, both as to the observations on the origin of these mutants and their behavior after they have come into being, and further, what success subsequent investigators have had in supporting De Vries's evidence, and how far they have extended his conclusions. In the first place, it may be remarked that the conclusions as first published in 1901 and 1902 were not the outcome of any hasty experiments and ill digested data, but were the result of seventeen years of the most careful and painstaking work, and a fine example of the best kind of quiet, faithful research, removed from the rush of affairs and the demand for immediate results, the final conclusion of which fully warranted the time and labor expended.

As is well known, Prof. De Vries found in Lamarck's evening primrose—*Oenothera Lamarckiana*—a plant most favorable for observation, though his conclusions are not based on that form alone. The most carefully guarded pedigree cultures were made from the true *Lamarckiana* type, and the astonishing result developed that among the offspring of these certain forms, to the number of about four per cent, showed new and striking differences. In all, more than a dozen new forms were obtained which, if they could be bred at all, bred true to their new characters and did not revert to the ancestral *Lamarckiana*; these were the mutants, the new elementary species, which had sprung suddenly in a saltatory fashion from the parent stock. The great importance lies in the fact that they were entirely constant to their new characters, and were thus not in the class of the merely unstable varieties. It must be remarked that time alone, many generations, of carefully guarded cultures in which accidental crossing was an impossibility, together with unimpeachable records, could adequately establish this momentous fact, that here was a new species, a new form, or whatever you may elect to call it, which had, sprung all in one jump from its parental stock. De Vries, then, was the first man who ever saw a new type of organism come into the world and who recorded its advent.

You naturally ask how unlike were these new forms, a question which is difficult to answer without actual illustrations. However, it may be said that many of them were different enough from their parent stock to be admitted by taxonomists to come within the definition of new species, as species are regarded at the present time. The differences are not the question of mere stature, but of the whole habit of the plant and of the details of the form of both leaves and flowers. But to repeat, it really makes no odds whether the differences are of such quality that they must needs be recognized as specific by taxonomists; what is important is that they are differences which do not intergrade one with another and which are inheritable in the second, third, and subsequent generations, and that no tendency to revert to the parent form is to be observed.

The results of De Vries have been verified by cultures in this country of his own and of other stock, so that there can be no question that this Lamarck's evening primrose behaves in its manner of mutation the same here as elsewhere. More than that, other mutating forms have been discovered, and by the application of biometric methods much that is important regarding the relative variability of mutants and their parent stock has been determined. Besides the actual experimental work, the history of Lamarck's evening primrose has been traced back for more than a century and a mass of inferential data is being accumulated which helps to support the main conclusions. Important as all these advances are, the most brilliant result is that obtained along the lines of the induction of mutations. By the injection into the developing ovary of a plant allied to Lamarck's evening primrose of reagents which might produce a chemical or osmotic effect upon the cell contents, MacDougal has actually succeeded in inducing mutations. The seed grown from the stimulated plant may produce forms quite distinct from the parent type and, what is essential, the mutations thus induced are constant to the second and third generations. That such a result can be obtained is simply astounding when one considers how firmly bound an organism is by its heredity. It would appear that a tremendous shock had been given the plant at a critical period in its life history which has enabled or forced it to break down some of the minor barriers imposed by its hereditary tendencies and to erect new ones, which circumscribe its offspring as the original ones did its parent. As to the precise nature of this shock we can at present only speculate, but it is permissible to suggest that it is perhaps of the nature of the rearrangement, in a chemical sense, of the protoplasm of the cells of the sexual generation. As to the natural production of mutants, given such a conception of the nature of the process involved, it is possible to suggest various ways in which it might have been brought about.

The line of departure of mutants from the parent type is not in any one direction, and the manner of variation appears to be wholly a matter of what we are pleased to call chance. As has been said, De Vries obtained more than a dozen different forms. Some of the mutants, we may say, are probably destined to failure, others perhaps are better placed, at least in new environment, than the parental type and might conceivably stamp it out in time. What the criteria of success or non-success may be is a matter upon which no one would care to give an opinion, but I have in mind the fact that one of the mutants of Lamarck's evening primrose has a tendency to germinate somewhat more quickly than the parent form, and the seedling grows a little more rapidly; it is conceivable that some slight advantage of this sort might be the crucial point. However that may be, it is here that we can apply the Darwinian concept of the struggle for existence, a struggle, however, not between single individuals, as the idea of continuous variation would imply, but the struggle between great numbers of individuals, whole groups of elementary species. The great contrast between Darwin and De Vries is the contrast between the slow and continuous accretion of variations implied by the former and the sudden jumping or saltatory variation insisted on by the latter. By such means as De Vries maintains the process of evolution might take place with far greater rapidity than by Darwin's method, for, generous as the geologists are in their allowance of time for the development of organic life on the world, it has always been difficult of conception how even the countless ages granted could compass the enormous development of the highest organic types from simple forms. To maintain that De Vries's theory is entirely complete, and must be the only means of the origin of new forms, is unnecessary. None but the extremist would go to such a length; it is not at all necessary to assume that the means to a similar end must necessarily be similar. What may be maintained, and properly so, is that mutation constitutes one way, at least, by which new forms of organisms may arise on the world's surface. New forms, in the sense of the new combinations of old characters which come into being by reason of stable, non-reverting hybrids, are known to have originated, but such new forms imply of course the pre-existence of varied types, and do not have to do with the question of the origin of new characters.

It is not in the order of things that a new theory of such import as the Mutation Theory should not find opponents. These I think may, in the main, be grouped in three classes. First, the critics who doubt the evidence, who can be answered by referring them to the printed records, and recommending a repetition, as careful as the original work, of the experiments which have led to the new point of view. Second, those who quibble concerning terms, and this type I think constitutes the majority, who will likely suffer the fate that is usually meted out to quibblers, that of being ignored. Lastly, those opponents who, while they may not doubt the accuracy of the work, doubt the conclusions on philosophical grounds. These are the critics whom the advocate of the De Vries theory must welcome, and who will arrest his sober attention, for they will stimulate him to accumulate more and more evidence to support his position. Even were I able to analyze adequately the controversial side of the question for you, it is obvious that time scarcely allows, and I will, in consequence, state frankly that the account which I have presented is from the standpoint of an advocate of what the Mutation

* Abstracted from a lecture on "Botany," delivered at Columbia University in the series on Science, Philosophy and Art.

Theory teaches, and add that I am not aware that any experimental work has controverted it. Let me say, however, and here I wish to speak for myself alone, that I cannot see it makes great odds whether fifty years hence or five years hence we accept the Mutation Theory just as propounded by De Vries. The great point is that an advance has been made, the most important advance since the time of Darwin, by way of helping to elucidate one of the great questions in which man is interested. It is not to be supposed that we have as yet any final answer to this question; final answers are not indeed the goal of any one scientific research. It was Sir Isaac Newton, I think, who said that the seeker after ultimate causes did not show the true scientific spirit, and he was right. What we have is one of the proximate causes demonstrated to a degree which had not been previously attained. A scientific theory is like an organism, it grows and it may also propagate itself, and all the theories of evolution from Lamarck to De Vries, and those that will follow, will themselves be an example, as it were, of the principle that they teach. A theory starts life an intellectual pigmy, may develop, if it have the vitality, into a veritable intellectual colossus, and, after it has run its course, may leave behind its offspring. It is not a cause of reproach but rather of congratulation that the scientific theory of to-day may be discarded to-morrow, for no theory will be abandoned until a better one has been brought forward to take its place, one which can explain the facts in a way more satisfying. Change in such a case is progress, and since science must of necessity be always progressing so also must it be always changing.

THE POULSEN WIRELESS TELEPHONE.

SYSTEM OF THE GERMAN WIRELESS TELEGRAPH COMPANY.

BY OTTO NAIRZ.

It is now possible to give a detailed description of the apparatus for wireless telephony, which has been adopted by the Gesellschaft fuer drahtlose Telegraphie (Wireless Telegraph Company).

The arrangement of the sending station is illustrated by Fig. 2. K_1 and K_2 represent the plugs by which the apparatus is connected with a high voltage, direct current commercial circuit (or other similar source of electricity). For convenience of reference the sending apparatus may be divided into three circuits, designated as A_1, A_2, and A_3. The circuit A includes the ampere meter A_1, two choking coils D_1 D_2, and six arc lights, of which only two are indicated in the diagram by the crosses I I. These arcs serve as generators of an oscillating current of high frequency.

The negative pole of each arc is a carbon rod, but the positive pole is a copper vessel, with a concave bottom, filled with water (Fig. 3). By this device the arc is kept at a low temperature, a condition which is essential for the maintenance of undamped or uniform oscillations. The water-cooled arc is an improvement on Poulsen's arc inclosed in hydrogen and kept cool by the high thermal conductivity of that gas. The oscillatory system A_2, connected in parallel with the arc lights, includes a second ampere meter A_2, a coil Lp, and a condenser C. The condenser is composed of semi-circular metal plates, separated by intervals of a few millimeters and immersed in oil. The odd-numbered plates are connected together to form one surface of the condenser and the even-numbered plates are similarly connected to form the oppositely charged surface. One series of plates is fixed but the other series can be turned about the axis of the cylinder in order to vary the capacity of the condenser. This capacity is greatest when the plates completely overlap so that the whole condenser forms a hemicylinder and it is least when they do not overlap at all so that the condenser forms a complete cylinder, half of which is occupied by one and half by the other series of plates. By this device the period of electrical oscillation can be varied greatly and adjusted accurately to any desired value. The discharge of a condenser is always of oscillatory character, the period of oscillation depending on the capacity and the self-induction of the circuit. In this case the period is practically determined by the capacity of the condenser and the dimensions of the coil and is of the order of magnitude of a millionth of a second.

The irregular flow of the direct current through the water-cooled arcs produces in the oscillatory circuit a true undulatory or alternating current, the condenser being charged alternately in opposite senses, and discharged about a million times per second. The arcs are drawn into sympathetic vibration, the direct current which traverses them never being reversed, but fluctuating between a maximum and a minimum value. The direct current, in addition to feeding the arcs, furnishes to the oscillatory circuit exactly the amount of energy that is dissipated in the form of heat and otherwise. Hence the oscillations suffer no damping but maintain their initial amplitude unchanged.

The coil Lp, of the second or oscillatory circuit A_2, is the primary coil of a transformer of which the

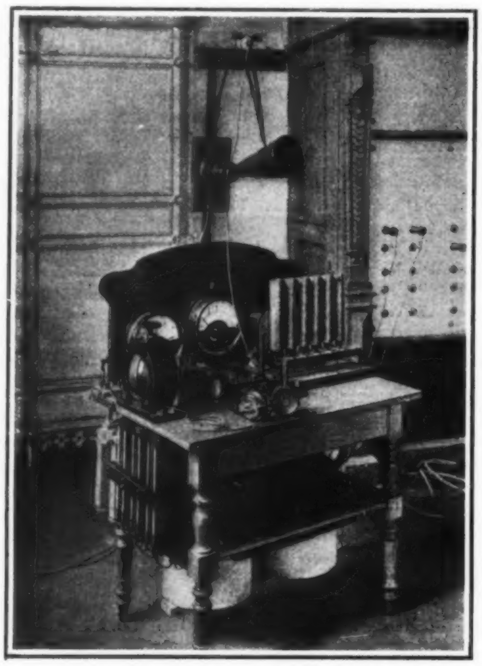


FIG. 1.—THE POULSEN TELEPHONE SYSTEM.

secondary coil Ls is included in the third or aerial circuit A_3, which also contains a grounded aerial wire, an ampere meter A_3 and a coil of variable self-induction Lz. Hence very rapid oscillations, synchronous with those of the oscillatory circuit, are induced in the aerial circuit. In order to give maximum intensity to these forced oscillations, it is necessary to bring the natural periods of the two circuits into unison. This turning may be effected by adjusting either the turning coil Lz or the variable condenser C.

As a result of the arrangement so far described the aerial circuit and its antenna are traversed by an oscillating current of very high frequency and absolutely constant amplitude. The addition of a key is all that is required for the transmission of aerial messages by the Morse code. In order to send telephonic messages however, it is necessary to superimpose upon the exceedingly rapid oscillations of the aerial waves slower oscillations, of the periods of sound waves. This is accomplished by means of a local battery (not indicated) and a microphone M, connected as a shunt around the coil Ls. The resistance of this shunt varies as the carbon grains of the microphone are more or less compacted by the diaphragm vibrating under the influence of the sound waves produced by the speaker's voice. Hence, of the high frequency current induced in the coil Ls a proportion depending on the character of the sound passes through the microphone circuit and the remainder traverses the aerial circuit, producing corresponding fluctuations in the intensity of the high frequency aerial waves which are sent out by the antenna and received at the distant station.

The main circuit of the receiving apparatus (Fig. 4) comprises an antenna, a grounded aerial wire, and two tuning coils, L_1 and L_2. The upper coil L_2 is employed for approximate tuning. The lower coil L_1 is composed of a fixed part and a movable part, by the rotation of which the self-induction of the compound coil can be varied and the period of the receiving circuit brought into exact agreement with that of the high frequency aerial waves coming from the sending station. A circuit arranged as a shunt around the compound L_1 contains a telephone T and a Schloemilch electrolytic detector Z with its local battery E. The connection with the battery is made by means of the variable resistance shunt W.

The Schloemilch electrolytic detector is a vessel containing dilute sulphuric acid and two electrodes, one of which, the cathode, terminates in an exceedingly fine point. As the cathode is very small and also covered with fine bubbles of hydrogen set free by electrolysis, the resistance of the electrolytic detector is very great and the current which traverses it is correspondingly feeble. But if an alternating current of high frequency also traverses the detector cell, the hydrogen bubbles are dissipated and the resistance is diminished to an extent determined by the intensity of the alternating current, and consequently the strength of the direct current from the local battery is increased in the same proportion. Hence, when the aerial electric waves and the high frequency oscillations imparted by them to the receiving apparatus, including the electrolytic detector, are caused to vary in intensity by speaking into the microphone at the sending station, corresponding variations are produced in the local current through the detector and the telephone, and the latter reproduces the spoken words, with all the characteristic overtones of the vowel and other sounds (Fig. 5.) As long ago as last December spoken sentences and a song by the tenor Caruso, as reproduced by a gramophone placed near the microphone, were satisfactorily transmitted to a station two miles away and spoken series of numbers were recognized at a much greater distance (24 miles).

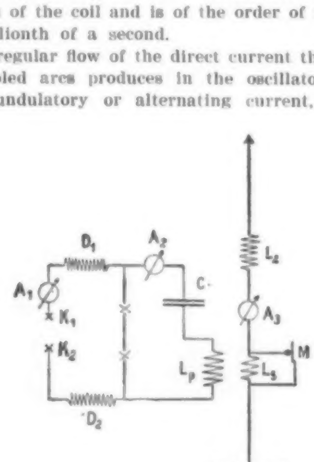


FIG. 2.—DIAGRAM OF SENDING APPARATUS.

FIG. 3.—WATER-COOLED ARC.

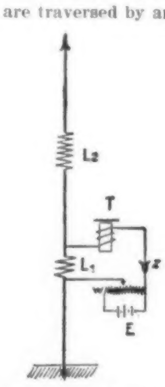


FIG. 4.—DIAGRAM OF RECEIVING APPARATUS.

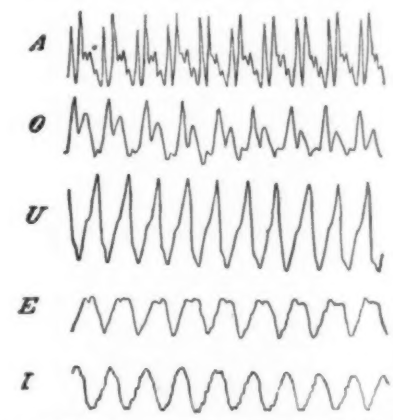


FIG. 5.—VOWEL CURVES AS RECORDED ON THE PHONOGRAPH.

Fig. 1 shows the compact sending and receiving apparatus exhibited at Prof. Slaby's Easter lecture before the German Emperor and Empress. On the right are the six arc lamps, in the middle are the telephone receiver, a switch by which the aerial wire can be connected with either the sending or the receiving apparatus, and the knob by which the variable condenser is turned.

The condenser itself is under the table, together with a shunt resistance for the lamps and the two choking coils which prevent the escape of the oscillating current to the service mains. On the left of the table is the compound tuning coil L_1 of the receiving circuit. At the back are the three amperemeters. The microphone, with its horn, is suspended over the table.

At present a station cannot be used simultaneously for sending and for receiving. Informal conversation, with quick replies and interruptions, is impossible. The listener must wait until the speaker has finished his statement or question and followed it with a certain spoken signal. Then both persons move their switches, converting the former sending station into a receiving station and conversely, and allowing a reply to be sent and received.

The disturbing effect of induction from neighboring wires, which is so great an annoyance in telephoning, is, of course, entirely absent from the wireless system in which the messages are heard very distinctly. The saving of the cost of wiring is another advantage. Yet it is not likely that wireless telephony will ever supplant the ordinary method, which is simple and more certain in operation. Hopeless confusion of messages would probably result from the working of numerous wireless stations within a small area. Wireless telephony, like wireless telegraphy, will find its true field in establishing communication between points that cannot be connected by wire.—Translated for SCIENTIFIC AMERICAN SUPPLEMENT from Prometheus.

PROTECTING COASTS AGAINST EROSION.

A NEW SYSTEM.

A NEW defense against erosion of sea coasts has been invented by Mr. de Muralt, a Dutch engineer. Instead of constructing embankments, or groynes, he paves the eroding beach with flagging of reinforced concrete. The flagstones are 36 inches square and from 2 to 3 inches thick, reinforced with half inch iron bars

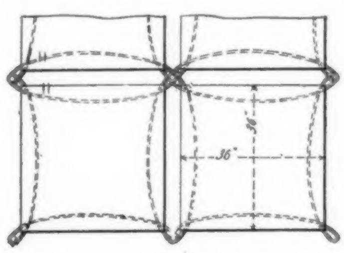


FIG. 1.

bent into an eyelet at each corner (see Fig. 1). These flags are cast on the shore, near the spot where they are to be placed, and are then assembled and bolted together with eyeleted bolts in lots of 400. This gives a square with a side of twenty flags, and measuring, with the small spaces or channels between each slab 66 feet square. The assembling is done on the beach at low water mark. This large square is placed in position by means of a float. This float consists of 144 watertight boxes each 6 feet 3 inches square by 16 inches high, made of one inch wood reinforced with blocks at each corner. These corners are pierced so that the boxes may be connected together with steel wire and bolts (see Fig. 2). A number of iron rings (Fig. 3) are placed on the float as shown in Fig. 4. In the center of each ring is an iron pipe C, 6 inches in diameter, and 16 inches deep. Four wings, A, also 16 inches deep and 2 feet long, connect it with a marginal ring R. These wings have slots (B, Fig. 5) cut in them so that they may fit over the steel wires which connect the boxes, and by fitting down between the boxes hold the pipe C in position, while the ring R, resting across four boxes, gives support. There are 121 intersections of the channels between the 144 compartments of the float and over 14 of them winches are placed (Fig. 5).

The float is assembled over the square of flagstones. Ropes are attached to the loop Z, Fig. 3, and are passed through an eyelet of a concrete slab, over a roller D, and to the winch. They are guided on to the winch by passing through one of the eight holes K (Figs. 3 and

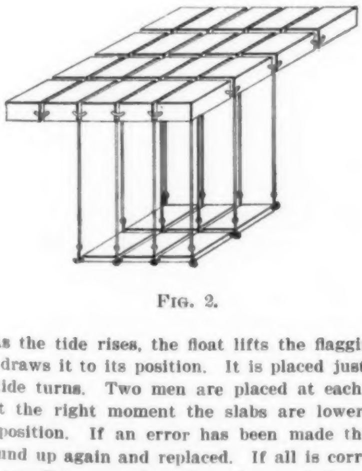


FIG. 2.

5). As the tide rises, the float lifts the flagging, and a tug draws it to its position. It is placed just as the high tide turns. Two men are placed at each winch, and at the right moment the slabs are lowered into their position. If an error has been made they may be wound up again and replaced. If all is correct, the ropes are disconnected at Z, and wound through the eyelets and the iron ring on the winch.

This facing of concrete slabs effectually prevents any further scour of the tide. The interstices between the flagging are soon filled with sand and pebbles, and the whole mass is practically permanent. So far from eroding, the concrete soon becomes coated with seaweed and barnacles.

The inventor considers that this method of protection is fifty per cent cheaper than the present groyne method.—Abstracted from Der Ingenieur.

ALUMINIUM AND BAUXITE.

MORE than seventeen million pounds (17,211,000) of metallic aluminium were consumed in this country during last year, according to Mr. W. C. Phalen, of the United States Geological Survey, whose statistical report on the production of aluminium and bauxite has just been published by the Survey as an advance chapter from Mineral Resources of the United States, Calendar Year 1907. This is an increase of 2,301,000 pounds over the consumption in 1906, which amounted to 14,910,000 pounds. The great increase in domestic production that was predicted in the early part of 1907 was not realized, and the failure of the predictions is attributed by Mr. Phalen, in large part at least, to the falling off in demand toward the close of the year as a result of general business depression.

The output of bauxite, which finds its most important use as raw material for the production of metallic aluminium, increased almost 30 per cent in quan-

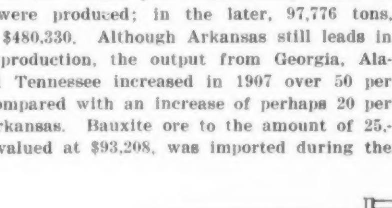


FIG. 4.

tity and a little over 30 per cent in value in 1907 as compared with the quantity and value of the output in 1906. In the earlier year 75,332 tons, valued at $368,311, were produced; in the later, 97,776 tons, valued at $480,330. Although Arkansas still leads in the total production, the output from Georgia, Alabama, and Tennessee increased in 1907 over 50 per cent, as compared with an increase of perhaps 20 per cent in Arkansas. Bauxite ore to the amount of 25,066 tons, valued at $93,208, was imported during the year, making the consumption of bauxite in 1907 amount to 122,842 tons, valued at $573,538.

In addition to its use in the production of metallic aluminium, bauxite is in demand for the manufacture of aluminium salts, artificial abrasives (alundum), and bauxite brick. This last use is of very recent date. The chief value of the bricks lies in their resistance to the corrosive action of molten metal at high temperatures, and hence they find application in basic open-hearth steel furnaces, in furnaces for refining lead, in copper reverberatory furnaces, and in the lining of rotary Portland cement kilns.

Mr. Phalen reports a decrease of 33 per cent in the

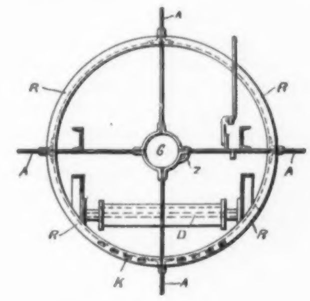


FIG. 3.

quantity of alum produced in 1907 (10,404 tons, valued at $361,900) as compared with the production of 1906 (15,613 tons, valued at $450,125). On the other hand, the quantity of aluminium sulphate produced in 1907 (106,821 short tons, valued at $2,008,046) increased about 20 per cent over that in 1906 (89,246 short tons, valued at $1,613,050). The imports of aluminium salts, including alumina, aluminium hydrate (or refined bauxite), alum, alum cake, aluminium sulphate, aluminous cake, and alum in crystals or ground, amounted in 1907 to 1,562 short tons, valued at $35,191, an increase of about 32 per cent in quantity and of about 52 per cent in value over the imports in 1906 (1,183 tons, valued at $23,193).

Mr. Phalen's statistical report includes notes on the domestic bauxite deposits and on new industrial developments in the aluminium industry. The bauxite deposits of Tennessee, a State which appears for the first time as a producer, are briefly described, as are also those of the new Georgia field, an account of which was published by Mr. Otto Veatch, of the Geological Survey of Georgia, in one of the technical journals early in April. Copies of Mr. Phalen's paper may be obtained by addressing the Director of the Geological Survey, Washington, D. C.

According to a consular report, the municipality of Antwerp has decided to construct a new drydock to receive vessels of the largest size. The site chosen is at the end of the Lefebvre Dock, close to the entrance to the Royers Lock. The dimensions of the new dock have not yet been definitely settled, but they will probably be as follows: Length, 754 feet; width of entrance, 88½ feet; and depth of water, 32¾ feet. The work will be carried out as quickly as possible, and will be completed in about three years.

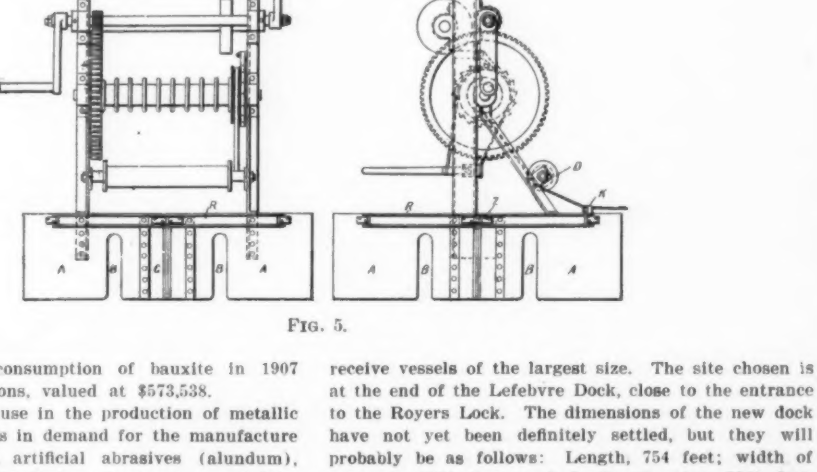


FIG. 5.

DEW-PONDS.*

AN INTERESTING ACCOUNT OF A CURIOUS PHENOMENON.

BY EDWARD A. MARTIN, F.G.S.

THE literature devoted to the subject of dew-ponds is of a very scanty nature, while those writers who have dealt with the subject differ considerably among themselves as to the principles, if any, on which such ponds are formed, and, also, indeed, as to whether the ponds have any right to be called "dew-ponds" at all.

In considering the subject, it is, of course, primarily necessary to recognize clearly how dew is formed, but even in what appears to be such an elementary matter as this there is not a unanimity of opinion. Many meteorologists still maintain the old theory, which is certainly the popular theory, that dew is formed by the precipitation of the aqueous vapor already existing in the lower layers of the atmosphere, when the radiation of heat from the earth has caused its surface to be in the condition to chill below the dew-point the layer of saturated air in contact with it. Precipitated moisture may appear in the form of dew, hoarfrost, mist, fog, or cloud, but in dew and hoar-frost there is precipitation without a cloudy intermediary. Freest radiation of heat from the earth's surface takes place when there are no clouds to reflect to earth the heat which it gives off at night. If there are no clouds, the chilling of the ground and of the layer of air in contact with it will be considerable, and the temperature may be reduced to the dew-point.

During the last twenty years the acceptance of Dr. J. Aitken's theory has been rapidly growing, that dew is really formed from the moisture which rises out of the soil with the radiation of heat, and that it is this which is precipitated when the air into which it passes has been so reduced in temperature as to be unable to hold it as aqueous vapor. If this theory be the correct one it would at once dispose of the suggestion altogether that dew-ponds are fed and filled by true dew, since the acquisition of dew could only then be obtained at the expense of itself by earlier evaporation.

Messrs. Hubbard, in their "Neolithic Dew-Ponds and Cattleways," give some details as to the formation of these ponds, although the source of their information is not stated. They say that there is at least one wandering gang of men, who will construct for the modern farmer a dew-pond which will contain more water in the heat of summer than during the winter rains. The space hollowed out for the purpose is first thickly covered with a coating of dry straw. The straw is in turn covered by well chosen, finely-puddled clay, and the upper surface of the clay is then closely strewn with stones. The margin of the straw has to be effectually protected by the clay, since if it becomes wet it will cease to attract the dew, as it ceases to act as a non-conductor of heat and "becomes of the same temperature as the surrounding earth." This would, of course, follow quickly if a runnel or spring were allowed to drain into the pond. The puddled clay is chilled by the process of evaporation, and the dry straw prevents the heat of the earth after a hot day from warming the clay.

It is very certain, however, that many alleged dew-ponds are not formed on this plan. This description, it will be observed, clearly presupposes that dew is formed out of the aqueous vapor already existing in the atmosphere, so that if Dr. Aitken's theory is correct, it would seem that a new name is needed to describe water that is precipitated out of the atmosphere in such a case, without the intermediate condition of mist or cloud. Such might be called "invisible mist." Some remarks by G. G. Desmond in the "Nature Notes Column" of the Daily News gave a different arrangement for the basis of the dew-pond. It was there stated that first a bed of concrete is laid down; this is covered with straw, over which is placed another layer of concrete. I have been unable to trace the authority on which this is based.

In a private letter from the maker of some ponds on the "Duke of Norfolk Downs" and on Amberley Mount, it is stated that the highest parts are chosen, as they are "more exposed to the weather" than lower down, the inference being that they are filled by the moisture-laden winds blowing in from the south-west, no consideration being given whatever to any artificial attempt to attract dew-precipitation. But as R. H. Scott says, dew can never appear when there is much wind, for the air can not remain long enough in contact with the soil for any material reduction of its temperature and consequent condensation of moisture to take place. (Int. Sci. Series, Vol. XLVI). The "weather" referred to can only, therefore, be mist or fog.

* Knowledge and Scientific News.

In 1877 Mr. H. P. Slade discarded the term "dew-ponds" in favor of "artificial rain-ponds," and scouted the idea that dew had any part in filling ponds at all. His remarks dealt practically with one pond, the greatest diameter of which was 69½ feet, which was constructed in 1836 at a cost of £40. It was bedded in the Thorpe Downs, near Loughborough, on the Berkshire Hills, at a height of 450 feet above the level of the sea. Being "fed from the heavens," this fact probably gave rise to its being classed as a dew-pond. The basis of this pond was stated to be first, a layer of clay about 12 inches thick (mixed with lime to prevent the working of earth worms), second a coating of straw, "to prevent the sun cracking the clay," and, thirdly, a layer of loose rubble. During an interval of forty years, till 1876, the pond had only once been dry. The exception was in 1854, and this resulted principally from the growth of rushes, whose roots struck through the clay bottom, causing leakage in what was otherwise "a waterproof bed." The straw was not held to have any particular effect in causing dew-precipitation, and the rubble, which would, of course, by the way, allow of the straw becoming saturated, was merely to prevent the hoofs of cattle trampling upon and perforating the clay, or puddle.

Gilbert White's mention of the little ponds on the downs around Selborne is an early reference to this class of ponds, but he does not actually call them "dew-ponds," so that the name may have come into use subsequently to his time. He says: "Now we have many such little round ponds in this district; and one in particular on our sheep-down, 300 feet above my house, which, though never above three feet deep in the middle, and not more than 30 feet in diameter, and containing, perhaps, not more than two or three hundred hogsheads of water, yet never is known to fail, though it affords drink for 300 or 400 sheep, and for at least 20 head of large cattle beside. This pond, it is true, is overhung with two moderate beeches, that, doubtless, at times afford it much supply; but then we have others as small that, without the aid of trees, and in spite of evaporation from sun and wind, and perpetual consumption by cattle, yet constantly maintain a moderate share of water, without overflowing in the wettest seasons, as they would do if supplied by springs. By my journal of May, 1775, it appears that 'the small and even considerable ponds in the vales are now dried up, while the small ponds on the very tops of the hills are but little affected.' Can this difference be accounted for from evaporation alone, which certainly is more prevalent in bottoms? or, rather, have not these elevated pools some unnoticed recruits, which in the night time counterbalance the waste of the day; without which the cattle alone must soon exhaust them?"

White then quotes Dr. Hales as remarking "that more than a double quantity of dew falls on a surface of water than there does on an equal surface of moist earth," but one must remark that this does not necessarily always hold good.

J. C. Clutterbuck, in 1865, said that in making such ponds an excavation was made in the chalk on the tops of the hills, from 30 to 40 feet or more in diameter, and from four to six feet deep. The bottom was "covered with clay carefully tempered, mixed with a considerable quantity of lime." This was "protected from the action of the sun and atmosphere by a covering of straw." After this "efficient and impermeable coating or puddle" is completed, "a layer of broken chalk is placed upon it."

It will have been noticed that in the Hubbard statement the excavated hollow is, in the *first* place, covered by straw, after which puddled clay is deposited thereon, with a strewing of staves on the top of that.

I should like to trace the wandering gang of men referred to in their work. I hoped to have hit upon some of them in the summer of 1906, when I found that a pond-maker, who seemed to be well known, was said to be at Alfriston. I interviewed him on the subject, but only found that the ponds which he made, whether on high or low ground, consisted of an excavated hollow, with a carefully concreted bottom. With thermodynamics he had nothing to do, nor did he show any inclination to advance the cause of science by building a scientific dew-pond. For £30 or £40 he would build one anywhere, but he would choose a site where runnels made their appearance in rainy weather.

In Johnson and Wright's "Neolithic Man in North-east Surrey," reference is made to the fact that some old Surrey people do not use the term "dew-ponds" at all for these remarkably constant supplies of water which are found on the chalk hills, but call them "mist-ponds," and the more inquiry is made into the origin of them, the more difficult it is to think of the majority of them as *dew*-ponds in the full sense of the word.

It has been attempted with some success to attribute the first formation of dew-ponds to the Neolithic peoples in England, and this has been the view of various writers on the subject, the necessity very early showing itself to such people of having reliable water supplies when besieged or shut up, even though for a short time, in their hill-camps. But, as Pitt-Rivers has pointed out, the time during which such sieges lasted could not have exceeded a day or two at most, and I can not help thinking that the ponds are more likely to have been constructed principally, if not entirely, for the watering of cattle, this being just as much a necessity in times of peace as in times of strife. The herbage found on the downs was then, there is no reason to doubt, just as sweet and wholesome as it is now, and our flocks are, by preference, still found in immense numbers on the Surrey and Sussex hills, although there are no marauding bands to waylay them nowadays in the lower lands near by.

It should be noted that Pitt-Rivers, in his notes on the Winklebury Camp excavations, 850 feet above sea level, speaks favorably of the idea that these highly-placed camps may have been watered by springs which then ran at a higher level than now. And, of course, if there were a probability of this, we should have here important evidence in favor of some dew-ponds having been filled at one time by springs. But this could never have been so in the case of those ponds which are really at the very summit of the downs. Gilbert White referred to the fact that the water-line in chalk was always found at the same level in all the wells in his district, although recent observations in Yorkshire go to show that the water-line follows the contour of the chalk hills. We know that since so many private wells and borings have tapped the chalk under London, the water level has been steadily sinking. The chalk is sometimes likened to a sponge in the way in which it soaks up water, and if this be the case, it will not yield surplus water until it has itself been saturated. But then, if the water-level be lowered, as we know it has been lowered, the chalk would still remain saturated if we grant it this soaking power, although above that water-level it would not yield a supply which could be tapped by well-sinkers. In the olden days therefore, it would not have been any more likely to have given rise to springs than now, and little more than the mere surface drainage, or that part which remained after percolation, would have gone to fill the ponds. Pitt-Rivers also points out that in many chalk districts "there are high springs which run only in the winter, when the hills have sopped up the winter rains, and retained them like sponges at the higher levels." ("Excavations in Cranborne Chase," Vol. II., p. 237.) But this can have no reference to summit-ponds, although the statement is quite true, and was probably considerably more so in former times, when forests and woods existed which have since beeen cleared. Still, if these springs merely flow because the water which supplies them can not sink into saturated chalk, then the ponds which they feed have no special reason to be called "dew-ponds" at all.

Yet, as White informs us, these strange little ponds on the tops of the hills are full when those in the bottoms are dried up; that is, in times when there has been a dearth of rainfall, and this, although it is admitted that the water level in the chalk has sunk as compared with earlier times. And, as Johnson and Wright say, even in our times the strange spectacle is sometimes seen "of carts being sent up hill to procure water for the granges and bartons in the vale." Besides, Mr. J. C. Clutterbuck refers to the fact, evidently admitted so recently as 1865, that the tops of chalk hills are often chosen for sites, where no surface-water except rainfall can furnish a supply. Therefore, as White says, there must be "some unnoticed recruits, which in the night time counterbalance the waste of the day."

What are these recruits? As the ponds have come somehow to be known as "dew"-ponds, it will be well first of all to consider whether dew is one of these recruits. H. V. Slade dismisses at once the possibility of its acting as such. It must be borne in mind, however, that he particularly referred to the one pond only, and in that the straw was laid on the clay or

puddle, and the only object of the straw was, according to his statement, with a view "to prevent the sun cracking the clay." He did not suggest that the straw was of use in keeping the water of the pond cool. But Hubbard says that the purpose of putting the straw *under* the puddled clay is to prevent the clay receiving heat from the earth which the latter has absorbed during the warmth of a summer day. At the same time the puddled clay is chilled by the process of evaporation, and the straw acting as a non-conductor, the moisture contained in the warmer air is deposited in the form of dew. In this way an empty pond will become filled without other assistance, the condensation during the night being in excess of the evaporation during the day, until, presumably, the margin of puddled clay around the pond becomes smaller and smaller, and dew deposited thereon ceases to recruit the pond.

In the meantime, as pointed out by Prof. Miall, although water itself is a bad conductor of heat, the surface of a pond would cool by radiation (very slowly), and in cooling would, of course, become denser. The layer at the surface would, therefore, sink, and give place, by convection currents, to water not yet cooled to the same extent and, therefore, less dense. The process of replacement being continued, the net result may be that the whole mass is cooled sufficiently to chill the superincumbent air below the dew-point. In this way a dew-pond, if built on the Hubbard plan, and granting the principles advanced by them, would, after becoming filled without artificial assistance, continue to receive dew (invisible mist, as I have called it), when partially filled, although the greater part of the clay were covered.

Clutterbuck, on the other hand, says that the water must, in the first place, be introduced by artificial means, but in this case we must remember that the straw was placed over the clay, and it was not claimed that the straw in any way attracted the deposition of dew. As Miall says, this seems to be decisive against the sufficiency of rainfall alone, in so far as such ponds are built after Clutterbuck's plan.

Clement Reid states that "the open downs, even in the middle of summer, receive much heavier dews than would be expected, or than are met with on the lowlands." But he adds that "thick sea-mists often cling to their top [of the open downs] for several hours after sunrise, while the plains below are already dry and sunny." This brings us to the question of mist acting as a recruiting agent, and one can not help thinking that this may be of material benefit to the pond.

The claim that dew alone is the great cause of the permanence of such ponds receives a shock from an experiment conducted by J. G. Cornish at Lockinge, in Berkshire, and recorded in C. J. Cornish's "Naturalist on the Thames." The temperature of the water in a dew-pond on Lockinge Downs on July 16, 1901, was 20 deg. F. higher than the temperature of the air. Dew could not, therefore, have been deposited, since the temperature would probably have been maintained throughout the night, but if not, the difference in temperature of the water and of the air would, at any rate, have been accentuated. This would be in accordance with the principle that water, although it takes longer to warm, yet when once it acquires a certain temperature it retains its heat without materially warming the air above it. Water has far less absorbing and radiating power than dry land, and, therefore, would have less effect on the air above it. Mr. R. H. Scott states that "as the specific heat of water is five times that of dry land, it takes five times as much heat to raise a given mass of water through a given range of temperature as it does to raise an equal mass of dry land."

Mr. Cornish also records that, on the other hand, five days of heavy dew in April and May, with no fog, raised the level of the same pond no less than 3½ inches. This record is so extraordinary that one hesitates to give it credence, and further similar observations are desirable. Attempts have been made from time to time to measure dew-fall, and Mr. G. Dines, in a paper "On Dew, Mist, and Fog," gave the average of his observation at 1.397 inches, or on the grass alone at 1.022 inches. "Making a liberal allowance for contingencies, it may, I think, be fairly assumed the average *annual* deposit of dew on the surface of the earth falls short of 1.5 inches." What, then, are we to say to a reported deposit of 3½ inches in five days?

One can scarcely help admitting that the positions of the ponds which are known, favor the fact that fogs do add a certain quantity of water to them. The experiments of Mr. Cornish, or, rather, of the shepherd whom he engaged, are very striking. After a night of fog, the surface of his pond was found on January 18 to have risen 1¼ inches; the next day, following another fog, gave 2 inches; and on January 24 an inch was measured. It was not recorded what was the principle on which the bottom of the pond was laid.

If mist be measured as a valuable agent in recruiting the ponds, then it is a fit subject for inquiry as to what steps should be taken to encourage the deposition of the mist as water. White admitted that an overhanging beech or other tree was of importance in connection with some of the ponds around Selborne. Clement Reid thinks that an overhanging tree on the side nearest the source of the moisture-laden currents of air is of importance. "When a sea-mist drifts in," in early morning or toward evening, "there is a continuous drip from the smooth leaves of the overhanging tree."

The position of the pond now becomes of importance, and if the pond has a high southern or southwestern bank, it seems to act in a favorable way in causing fog to precipitate its moisture.

The Sussex Downs are the home of the dew-pond, and many a time for the whole of a day I have walked through dense fogs which have rolled in from the sea, and have finally taken their flight, as from a jumping-off ground, along the northern ridge of the downs between the Dyke and Plumpton. The trees, where there are any, such as the Holt, near Clayton, will then be seen and heard dropping water on to the leaf-soil below, while one's own garments become damp and clammy.

One does not like to part from the idea that dew-ponds have been correctly so named, but there is no direct proof that they are so. On the other hand, there is a good deal to throw doubt upon its correctness, since no pond, situated as they are, could fail to receive a great deal of condensation from mists.

But I am strongly inclined to think that the use of straw may have a good deal to do with the attraction of moisture to a pond. It is used in India to produce a low temperature and so obtain ice in the open, at night time. Mr. T. A. Wise has described (Nature, Vol. V., p. 189) a method by which quantities of ice are obtained in the neighborhood of Calcutta. An excavation of the ground to the depth of two feet is made. This is filled with rice straw to within six inches of the surface, somewhat loosely laid. Shallow pans of porous earthenware are then filled with water, and as long as the air is comparatively still, the ice forms in the pans. The straw is a powerful radiator, and, being kept loose and dry, prevents the heat rising from the earth to the water in the pans. Heat is cut off both top and bottom, and it is stated that the temperature of the air in contact with the dishes is reduced some 20 deg. below that two or three feet higher up. This practice certainly seems to throw some light on the use of straw at home.

One thing, at any rate, is certain, that mists contribute largely to these ponds. What we need now is a scientifically-constructed pond on the Hubbard principle as a first experiment. At present I know of no other direct and unqualified statement as to what a dew-pond really is, how it is constructed, and why it attracts the dew, and it might, I think, be put to the test. Then if it were successful in collecting water, with no artificial introduction of a supply in the first place, meteorological observations might follow to show, if possible, the laws which were most potent in accomplishing it.

SIR HUMPHRY DAVY AS SEEN BY HIS CONTEMPORARIES.

The Royal Institution had a troubled infancy. Like the poor it was originally designed to succor, it suffered much in the outset from lack of nourishment. To add to its miseries, the little starveling was caricatured by Gillray, lampooned by Peter Pindar, and ridiculed by Lord Brougham, and it was literally in the throes of dissolution when new life was breathed into it by the opportune arrival, in 1801, of a small spare youth of twenty-two from Bristol, whom the managers had engaged at a salary of 100 guineas a year. The youth was Humphry Davy, who had acted as assistant to Dr. Beddoes, of the Pneumatic Institution, and who had already made some slight stir in scientific circles by his discovery of a characteristic property of nitrous oxide. In announcing his arrival to the managers, Count Rumford reported that he had purchased a cheap second-hand carpet for Mr. Davy's room, together with such other articles as appeared to him necessary to make the room habitable, and among the rest a new sofa-bed, which, in order that it may serve as a model for imitation, had been made complete in all its parts. Six weeks after his arrival Davy was called upon to lecture, and a descriptive paragrapher of the period thus chronicles his success in the Philosophical Magazine for 1801:

"It must give pleasure to our readers to learn that this new and useful institution, the object of which is the application of science to the common purposes of life, may be now considered as settled on a firm basis. . . .

"We have also to notice a course of lectures, just commenced at the institution, on a new branch of philosophy—we mean the Galvanic Phenomena. On this interesting branch, Mr. Davy (late of Bristol) gave the first lecture on the 25th of April. He began with the history of Galvanism, detailed the successive discoveries, and described the different methods of accumulating galvanic influence. . . . He showed the effect of galvanism on the legs of frogs, and exhibited some interesting experiments on the galvanic effects on the solution of metals in acids. Sir Joseph Banks, Count Rumford, and other distinguished philosophers were present. The audience were highly gratified, and testified their satisfaction by general applause. Mr. Davy, who appears to be very young, acquitted himself admirably well; from the sparkling intelligence of his eye, his animated manner, and the *tout ensemble*, we have no doubt of his attaining a distinguished eminence."

And what was of more immediate consequence, this confident assurance was shared also by the managers, for at a subsequent meeting they unanimously resolved "that Mr. Humphry Davy, director of the chemical laboratory, having given satisfactory proofs of his talents as a lecturer, should be appointed, and in future denominated, lecturer in chemistry at the Royal Institution, instead of continuing to occupy the place of assistant lecturer, which he has hitherto filled."

That such shrewd experienced men of the world as Sir Joseph Banks and Rumford, who were the moving spirits in the management of the institution and genuinely solicitous for its welfare, should thus entrust its fortunes, then at their lowest ebb, to the power and ability of a young and comparatively unknown man, barely out of his teens, seems, even in an age which was familiar with the spectacle of "a proud boy" as a prime minister, like the desperate throw of a gambler.

But Banks and Rumford had, doubtless, good reason for the faith that was in them. For a happy combination of circumstances had served to bring the Cornish youth within the range of many who could be of service to him in that search for the fame for which he hungered. His connection with the Beddoes brought him the friendship of the Edgeworths, and it is amusing to trace how the good-humored patronage of the gifted Maria quickly passed into amazement and ended in awe as her acquaintance with him ripened. Living in Bristol, he was at once brought into that remarkable literary coterie which distinguished that city at the close of the eighteenth century. Southey spoke of him as a miraculous young man, whose talents he could only wonder at. Cottle, the publisher, on one occasion said to Coleridge, "You have doubtless seen a great many of what are called the cleverest men—how do you estimate Davy in comparison with these?" Mr. Coleridge's reply was strong and expressive. "Why, Davy can eat them all! There is an energy, an elasticity, in his mind which enables him to seize on and analyze all questions, pushing them to their legitimate consequences. Every subject in Davy's mind has the principle of vitality. Living thoughts spring up like turf under his feet."

Davy's experimental work on "the pleasure-giving air" had made him known to the Watts and the Wedgwoods. Priestley, then in exile, and Hope, of Edinburgh, were greatly impressed with the philosophical acumen of the author of phosoxygen, and he had a powerful friend in his own countryman, Davies Gilbert, who succeeded him in the presidential chair of the Royal Society. There need be no doubt, therefore, as to the influence which conspired to bring Davy into what he termed "the great hotbed of human power called London."

IS YOUR KITCHEN SANITARY?

Kitchens must be light. Like any other workshop, the lighter they are the easier in which to work, and the lighter they are, the more openings, the easier they are to ventilate. Kitchen windows should be arranged to drop from the top as well as to be raised from the bottom, so that there can be a constant circulation of air in the upper part of the kitchen, the hot air passing out and the fresh air being carried in. With the hot air will pass out the odor of cooking and steam. A good sink is an important part of a kitchen, and now sinks are made in so many useful ways that a little time will discover many labor saving devices. A modern sink is made with a division in the center, which is an ideal thing for dish washing and many other purposes.

The porcelain lined sink is without question the most desirable. The old wooden sinks lined with zinc are things of the past. The water got under the zinc, as did also water bugs and various other insects. This is all overcome by the use of the iron sink thoroughly coated with porcelain. It is clean and inexpensive. The kitchen floor should be hardwood, but if not it should be covered with linoleum. The careful housewife insists on clean floors, but to tax her with scrubbing is not fair. The floor should be one that can be easily cleaned and quickly dried. Where linoleum is used or the floor is of hardwood it can be mopped up quickly with very hot water if a good mop wringer is used. This will remove the grease from the floor, and, because the water is so hot, it will dry quickly.—Success.

ELECTRICAL NOTES.

It was shown by Dr. J. A. Fleming in 1904 that a carbon filament glow lamp, having in its bulb a metal cylinder surrounding the filament carried on an insulated terminal, could be used in combination with a galvanometer or telephone as a wave detector in wireless telegraphy, owing to the emission of negative electricity from the incandescent carbon. Such a device was named by him "oscillation valve," and is a very sensitive long-distance receiver. Glow-lamp detectors of this type have been used as receivers in transatlantic wireless telegraphy and are also of use as receivers for wireless or radio telephony.

In a paper read before the recent Royal Society *conversazione*, Mr. J. E. Barnard brought out the fact that mercury vapor lamps have considerable advantages as illuminants for microscopic work, as their visible spectrum consists chiefly of three bright lines, one each in the orange, green, and blue-violet. It is therefore possible, by using suitable absorbent color screens to transmit only one bright line of the required color, the remaining ones being absorbed. The source of light then becomes truly mono-chromatic, and is practically of one wave length. Even without color screens there is marked increase of resolving power.

The single-phase system of electric traction is extending in Europe and especially in Germany. The minister of the Prussian State Railways has decided to electrify two sections of the main lines running north from Leipzig, namely, the Leipzig-Halle section, 18¾ miles, and the Leipzig-Magdeburg division, 75.6 miles. The principal reason for the electrification of these lines appears to be the fact that there is available a large deposit of brown coal between Leipzig and Halle which is not a suitable fuel for steam locomotives, but which can be used to advantage in the boilers of a central electric power station. By the single-phase system it is possible to supply current to each of the lines mentioned from one central power house. The Prussian State Railways are now experimenting with a single-phase locomotive consisting of two trucks, each having two axles and joined by a short coupling. Three of the axles are driven by Winter-Eichberg motors through tooth gearing having a ratio of 1 to 4.2. At a speed of 500 revolutions each motor develops 250 horse-power. The line current is 6,000 volts and the high-tension apparatus on the locomotive is contained in a compartment which can only be entered when the contact bars are off the line; otherwise the door is bolted. Bavaria is also introducing the single-phase electric traction on a number of railways where the power will be derived from waterfalls and dams. One of the latest projects for the electrification of steam lines in England is that on the London, Brighton & South Coast Railway, which is also to use the single-phase system.—The Railway Age.

At a recent meeting of the Académie des Sciences, M. Bouquet de la Grye exposed his views as to the use of radio-telegraphy in sending hour-signals over the globe, especially for giving the time for vessels, seeing that the observations cannot always be made on shipboard and in any case are not always accurate. It is possible that electric waves can be sent to all parts of the globe so as to indicate the time from a chosen meridian, such as that of Paris. The waves sent from the Eiffel Tower can now cover a distance of 1,200 miles even with the present apparatus of very low power, and when the new apparatus is installed, there is no doubt that this will be doubled at the very least. But we may cover even greater distances by using the peak of Teneriffe, whose altitude is 12,240 feet, and letting down a mast wire which would run to the sea over a distance of 8½ miles. No doubt we can thus increase the present distances tenfold, and the signals would thus go to the Antipodes. The present idea is not to send messages as at present, but only special signals of an exceptional intensity. These would be sent once a day, and would give the hour of the first meridian. It would probably be best to send the signal at night, so as to lessen the atmospheric effects, and it would pass around the earth in both directions from the starting point. At the same time there should, of course, be an international agreement, so that but one such signal would be used, avoiding any confusion in the matter. The feasibility of such a system has been accepted by the president of the Radio-telegraphic Commission, M. Becquerel, as well as by Admiral Gaschard, chief of the Naval Technical Service, who is now directing the communications from Paris to Morocco, but the latter thinks that it could be carried out even more easily. Although the mean slope between the peak of Teneriffe and the sea is but 13 deg. 30 min., the mass of the mountain itself may be a hindrance for the waves. Perhaps a sea beach of four miles length, far from any mountain, would be preferable for the purpose. The beach of Guetn'dar, of Senegal, in the trade-wind region, is a favorable place for all conditions. In general, the use of such signals would give a great security for navigation. It would suppress long calculations on land and this would be one of the great benefits. To make a practical beginning, he proposes commencing with the Eiffel Tower, using the present plant, except to increase the mast wires and the power. A time signal would be sent at midnight, and would no doubt be perceived all over the Atlantic. He also proposed that a motion to this effect be laid before the Minister of the Marine. The matter has been referred by the Academy to a special committee appointed for the purpose.

SCIENCE NOTES.

A typhoid fever survey to determine the means, aside from domestic water supplies, by which this disease is transmitted is to be conducted in Pittsburg at the expense of the Russell Sage fund. The introduction of filtered water to the city ought to permit other sources of infection to be traced readily.

Mr. F. W. Aston exhibited at the recent Royal Society *conversazione* a tube containing helium at a pressure of about 3 millimeters. The tube was provided with aluminium electrodes, the cathode being a large flat plate. A continuous current of low density was passed through. Under these conditions the "Crookes dark space" was ill-defined and filled with a greenish glow, while next to the cathode was seen a narrow region of intense darkness sharply defined. The fall of potential across this "new dark space" appeared to be invariably about 30 volts.

Mr. J. Gray has devised an instrument for measuring the color of the hair, eyes, and skin. The instrument is a simplified form of the Lovibond tintometer. An aperture in the stage at the end of the instrument is placed over the hair, eye, or skin whose color is to be determined. The observer then looks through the tube, and interposes Lovibond's standard color glasses in front of a white surface placed at one side of the aperture in the stage. When the color of the light transmitted through the glasses is exactly equal to that of the hair, or the like, under observation, the exact composition of the color is determined from the readings on the standard glasses.

France has not quite 18 per cent of forest—three-fifths of an acre per capita. This is enough to produce only one-third of the home demand. The country imports annually $30,000,000 worth of wood, and pays $6,000,000 duty and $10,000,000 freight for it. This wood comes from Russia, Sweden, Norway, Austria-Hungary, Germany, and America. Of the 23,500,000 acres of French forests the state owns 2,707,000, and the departments and communes 3,470,000. Since 1827, when the forest code was passed, the state and communal forests have been under management. The state forests yield a clear profit of $4,737,250 a year, or $1.75 per acre; $0.95 is spent for the management of each acre every year. The best managed state forests yield about 40 cubic feet per acre a year, which is low compared with the yield of some other European forests, such as those of Prussia, Saxony, or Würtemberg.

A demonstration of the curvature of the earth's surface was described by Robert M. Brown before the American Association for the Advancement of Science. The paper recorded an observation by the writer on the curvature of the earth at Lake Quinsigamond, near Worcester, Mass. A board two feet square, divided vertically into a black upper surface and a white lower one, was set up with the union of the two surfaces at a certain height above water level. On an island about 4,000 feet away a white bar was erected, parallel to the water and at the height of the horizontal line of the first piece of apparatus. In line with these two and about 4,000 feet beyond the second piece, a telescope was set at the given height above the water. On sighting through the instrument the bar was projected against the top of the board. A scale suspended from the bar showed the amount of deviation from a straight line. From this reading the size of the earth was deduced.

With the exception of the sea-otter of the North Pacific, which has long been known to feed on sea-urchins and other hard-skinned marine creatures, it has hitherto been supposed that otters fed almost exclusively on fish. It appears, however, from a paper by Dr. Einar Lönnberg that this is not the case with an African clawless race (*Lutra capensis hindei*), whose food consists mainly of crabs, supplemented by an occasional egg or young bird. This otter is, moreover, much less agile in the water than the European species and its immediate relatives, being unable to remain so long beneath the surface, and when hunted seeking refuge on shore rather than in what has hitherto been considered its natural element. In fact, there is every probability that it is unable to capture fish, except when they are penned up in small pools during the dry season. Correlated with this crab-eating habit is the blunter character of the crowns of the cheek-teeth, which lack the sharp cusps of our own otter. This crab-eating otter does not enter into competition with the fish-eating species, and it is therefore interesting to find that a representative of the latter inhabits the same rivers.

TRADE NOTES AND FORMULÆ.

To Clean Marble Slabs.—The best way to clean marble slabs and remove oil or grease is to prepare a thin pulp of Spanish white, mix with benzine or petroleum ether, spread the mixture over the marble and allow it to remain there, covered with a damp cloth, for six or eight hours. If the spots are old, the process must be repeated several times. If benzine alone does not produce the desired effect, a little chloroform should be added. To polish the slabs, use a mass of washed emery and tin putty, spread on a linen rag. The effect is surprising. To cement marble slabs, mix 4 parts of burnt gypsum, 2 parts of powdered oyster shells, and 1 part of powdered gum arabic. When required for use, stir a little of this mixture to a thick paste with water and white of egg, spread it in a thin layer over the fractures, press the pieces together, and leave the article for 24 hours in a medium temperature.

To Purify Honey.—André describes a method of purifying honey, which leaves nothing to be desired in respect to speed and cleanliness, and which causes no deterioration in the color, taste, or smell of the honey. About 25 pounds of honey at a time is diluted with half this weight of water, and boiled over a gentle fire with a paste obtained by twirling three sheets of white blotting paper with water till the paper is reduced to very fine fibers. The mixture is allowed to cool, and then put into a woolen straining bag previously moistened. The honey runs off as clear as wine, and when the remaining paper pulp is washed out, the dark yellow wine-colored fluid is evaporated to the required consistency in a vapor bath. Excellent purified honey can also be obtained by diluting the honey with twice its weight of hot water, digesting with charcoal, freshly annealed, coarsely crushed and freed from fine powder, straining and evaporating with a gentle heat.

To Remove Grease Spots from Printed Paper or Manuscript, Lithographs, Copper Engravings, etc.—Place the soiled sheet inside a book, if it is not already bound in a book. Then sprinkle the spot uniformly on both sides with finely sifted, warmed white bole half a line thick, put the book in a press or weight it down with stones. In twenty-four hours clean the spot carefully and sprinkle it again with fresh warm bole, which must likewise be left for twenty-four hours in contact with the spot. The latter will then have entirely disappeared. A thick paste prepared from burnt magnesia or white bole with benzol or benzine is also very useful for removing grease spots from paper or clothes. It is applied to the spot, and when dry brushed and scraped off, after which no trace of the spot will be found.—Der Industriöse Geschäftsmann.

Rubber Dressing for Belts.—Cut India rubber into small pieces and dissolve with 5 parts by weight of turpentine oil in a small iron well-covered crucible at a temperature of 50 deg. C. (122 deg. F.) over a coal fire. As soon as the rubber is dissolved, add 4 parts by weight of rosin, stir, remelt, and add in the same way 4 parts by weight of yellow wax. While melting the mixture must be occasionally stirred. Then put 15 parts by weight of fish oil and 5 of tallow into a sufficiently large vessel, heat till the whole is melted, and add the first mixture warm, stirring all the while. Continue stirring till the mass is compact. The dressing should be used in the following manner: If the belts are old and brittle, apply the dressing freely with a brush on both sides in the sun or a warm room and leave them to dry. New belts, or belts that are still good, should like the previously treated brittle belts, be lubricated a little on the inside from time to time while in operation; in this way they will be rendered very durable, and will engage well on the pulleys, drums, etc. Cheap, old rubber waste can be used instead of India rubber; it should first, however, be boiled for a quarter or half an hour in soda lye, and 6½ parts by weight instead of 5 should be taken.—Neueste Erfindungen und Erfahrungen.

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